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DISSERTATION

SUR LA CAUSE
QUI CORROMPT ET NOIRCIT
LES GRAINS DE BLED
DANS LES ÉPIS;
ET SUR LES MOYENS
DE PREVENIR CES ACCIDENS.

Qui a remporté le Prix au Jugement de l'Académie Royale des Belles-Lettres, Sciences & Arts de Bordeaux.

Par Mr. TILLET, de Bordeaux, Directeur de la Monnoye de Troyes.

A BORDEAUX,
Chez la Veuve de Pierre Brun, Imprimeur Aggrégé de l'Académie Royale, rue Saint Jâmes.

M. DCC. LV.
AVEC PRIVILEGE DU ROI.

Phytopathological Classics

NUMBER 5

DISSERTATION ON THE CAUSE OF THE CORRUPTION AND SMUTTING

of the

KERNELS OF WHEAT IN THE HEAD

and on the means of preventing these untoward circumstances

Awarded the Prize by the Royal Academy of Literature, Science, and Arts of Bordeaux

by

M. Tillet, of Bordeaux, Director of the Mint of Troye

TRANSLATED FROM THE FRENCH

by

HARRY BAKER HUMPHREY

At Bordeaux

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INTRODUCTION

M. Mathieu du Tillet, one time Director of the Mint in Troyes, France, and author of numerous important papers on bunt of wheat and on the control of the Angoumois moth, was born in Bordeaux, France, in 1730, or thereabouts, where he applied himself during his spare time to the improvement of agriculture. In addition to his contributions pertaining to improved farm practice, he published important papers on the economic milling of cereals and especially that of wheat.

The amount of available published information concerning this scholarly and industrious man, who labored so effectively for the advancement of experimental science, is seemingly very meager. Nothing apparently is known concerning his early education, and we are left with the impression that, although a man of parts, he was modest withal, even to the point of self effacement. His published work would indicate that he must have been a man of unusual iniative and energy.

Single-handed and alone he applied himself assiduously to the development of the experimental method of solving problems in agronomy. Modern science owes to Tillet, and men of his scholarly instinct, a debt of gratitude that can never be paid. He lived at a time when men were much given to the more "gentlemanly pursuit of speculation" as to the cause of any natural phenomenon. The inductive and objective approach to problems in natural science had yet to be accepted as the truly scientific method of discerning and interpreting the facts.

Obviously, Tillet, at the outset of his career as an experimentalist, was not an agronomist, nor was he a botanist; he was, however, a born investigator. All he needed was a problem plus the opportunity and facilities for its solution. With unswerving zeal and fidelity to purpose he applied himself to the task of first putting under the hammer of relentless experimental test and analysis every

theory that up to his time had been heralded as a possible explanation of the cause of the then already well-known bunt of wheat. This he did with gentlemanly respect to authors and adherents alike of such hypotheses and deductions as rested on other foundation than that of proved fact.

He had first to clear his own thought of the trammels of deductive reasoning and speculation about natural phenomena; indeed, his masterpiece, *Dissertation sur la cause qui corrompt et noircit les grains de bled dans les épis* may be regarded, in a sense, as a history of the development of a scientific mind. It is probably true that when he first began thinking about the subject of his *dissertation* it was his intention to subject his materials to the acid test of well-checked experimentation. And yet, it is equally true that he was not wholly divorced from the temptation to draw conclusions from mere observation and *à priori* reasoning. Witness, for example, his treatment of the subject of ergot in cereals and grasses. After due consideration of the explanations advanced by Languis, Fagon, and others none of which was based on experimentation, he falls into the same pit and concludes that ergot is caused primarily by the sting of an insect. It is, however, conceivable that, had he devoted as much thought to the study of this disease as he did to that of bunt, he would have resorted to the experimental method with the same assiduity and patience he applied to his investigation of bunt.

Tillet did not succeed in discovering the fact, later disclosed by Prévost, that the primary cause of bunt was, in truth, a parasitic fungus; but he did very definitely prove that the disease resulted through contamination of the seed with the bunt dust. At no time did he suspect the true origin and function of this dust, but concluded that it was the active agent in transmitting a virus to the wheat plant, a poisonous principle that was self-perpetuating in its power to reproduce more of the same sort of virus-transmitting dust.

It is truly remarkable that here was a man who, without the certain advantage of training in agronomy and botany, had the genius to carry to successful issue research of such far-reaching importance. Without instruction and alone, he planned comprehensive experimental research on an obscure problem, and it is small wonder that with his lack of

knowledge of parasitism he failed to identify bunt with a parasitic fungus. He did, however, definitely prove not only the value, but the necessity, of the experimental method in research, and established forever the fact of the infective principle of bunt dust. Richly did he deserve the great honor accorded him by the Academy of Science of Bordeaux.

HARRY B. HUMPHREY

DISSERTATION ON THE CAUSE OF THE CORRUPTION AND SMUTTING OF WHEAT IN THE HEAD

BY

MATHIEU TILLET

TO THE KING

SIRE,

A signal distinction was, then, reserved for me, when one of your Academies favored and honored me with its votes for the second time. YOUR MAJESTY deigns to welcome my slight accomplishment and, by appearing under his auspices, permit it to be announced with that éclat hardly merited by writings of the first order.

The exceptional privilege you have accorded me I doubtless owe, SIRE, to the approbation of an enlightened group and to the important subject it has furnished me.

Occupied with the responsibility of serving your happy people, you make us all mindful of this great objective: useful researches interest you; the slightest undertakings that tend to benefit the public are supported by you; and this unmixed pleasure, which one experiences in service to mankind, is unquestionably a part of the pleasure of serving you.

What say, I, SIRE? Agriculture, the foundation of our riches, and the most precious of our resources, becomes for YOUR MAJESTY the subject of ceaseless solicitation; you examine its principles; you are the august Spectator of Experiments designed for its perfecting; and, without losing anything of the grandeur of the Monarch, you perform with zeal the simple functions of the Citizen.

ROME and FRANCE, in their best days, posterity will tell us, saw labor succeeded by victories and triumphs; the plow was guided by heroes. CINCINNATUS quit the purple and

returned to his fields, wearing a crown of gold, which the Roman army had bestowed upon him. LOUIS sometimes descended from the throne, and, crowned with laurels, presided over the culture of the soil.

I am with a very profound respect,

SIRE,

OF YOUR MAJESTY,

The very humble, very obedient servant and very faithful subject,

TILLET.

PRELIMINARY DISCOURSE

The different contributions that I bring together to-day under the eyes of the reader have been presented to the Academy at different times, and without its having been possible for me to express myself like a celebrated Writer by presenting my work at a single stroke. This Society, interested only in demanding research of concern to the public weal, proposed in 1750 the subject of this dissertation; it was absolutely new to me: I then felt that I should first examine how it had been treated up to that time, reserving to observe for myself wheat plants in 1751 and to follow them in their various stages of growth.

This first task led me to some useful information; but, still, it but meagerly satisfied me from the standpoint of my main object. In the hope that, through different experiments to which I could wholly devote myself, some light on the origin of the diseases of wheat might be revealed to me, I hastened, from the month of October, 1751, to take this instructive and exact method of compelling nature to explain herself.

But the time of the meeting expiring by the last of April, 1752, I was in a position to present to the Academy only the first part of this work, in the which my experiments were merely announced; the result of these same experiments being presentable for communication only after harvest and too late for its [the Academy's] judgment.

To this first part I presently added several observations in the form of a supplement. I had made these observations on my seed plots while the wheat plants were still standing and before I clearly saw the actual objective I sought to attain.

I awaited with impatience the decision of the Academy when it proposed anew the important subject in question, and invited the authors who had transmitted some memorials on the subject to add some new observations.

Consequently, I sought to confirm the results of experiments presented in the first part of this work; I repeated in 1752-3 those that had appeared to me to be decisive. I devised new ones; and, withal, laid out in order, I initiated a series of experiments relative to the two contributions that the Academy already had in hand.

This assemblage of various facts, I believe, related in great detail and accompanied wherever possible by considerations, became naturally the second part of my dissertation, and is, without question, the most interesting, as a whole, and the most valuable to me. To it I owe the distinction and approbation with which the Academy has honored me. It is of other plaudits, those of the plain husbandman, which, although less impressive, I would be most heartily appreciative should I be so fortunate as to win them. I have labored for this class of people, so dear to the State; I have occupied myself in their interests, that they, themselves, might be alert and gain from the fruits of my labor; I shall find them grateful if my researches profit them anything; any attention they may give them I shall regard as an eulogy; and I shall receive through their peculiar benefits the reward I most covet.

The contributions thus separately made are, then, reassembled for one and the same reading, in such manner that the last shall serve ere long to reveal the mistakes overlooked in the first. These contributions, not articulating with each other, in an exact manner, present an irregular and interrupted succession. They do not at all form that continuous thread, that chain of ideas that follows a well-coordinated plan without interruption.

But, as has been observed, this sort of imperfection in a work has something instructive about it; it presents naturally the progress of human thought in a new or little-known subject. It shows at first the readiness of the human mind to seize upon anything bearing the appearance of truth and its hesitation to reject completely a system that had baffled it, its subsequent discretion in the presence of false appearances, its ability to catch a truth-revealing gleam of light, its zeal in dissipating the clouds that still envelop it, its service, finally, in revealing the truth to the full light of day and in rendering it as evident to others as it truly is. A philosophical mind sometimes finds material for reflection in

that which appears to the common man hardly worthy of consideration.

The first part of this dissertation is divided into four chapters; it would have been tiresome for the reader without these moments of rest when he might catch his breath and relax his attention; I have even subdivided the third chapter, since it is more weighty than the others, and requires a particularly clear and equally distinct presentation of views.

CHAPTER I The first chapter is intended to summarize what different authors have said on the origin of the principal disease of wheat and on the remedies that should be applied. The scant agreement that obtains between them and the marked opposition one sometimes finds will be apparent. Sometimes the source of the disease is regarded as independent of the seed and the treatment of the seed rejected as useless; sometimes this source of corruption is not considered as at all foreign to the seed and its treatment has been adopted as a protective measure; here certain manures are excluded as dangerous; again, the whole is indifferently admitted as harmless; certain ones fear for the wheat the malignity of certain fogs; others pay no attention to them and fear only failure of fecundation, or an excessive humidity or a violent sunstroke on the water-soaked heads.

CHAPTER II It was necessary before discussing the various opinions advanced on the cause of the disease of wheat to cite first, as so many principles, some observations on which one may depend; to present facts that nobody can question, in order that those opinions, compared with some established facts, either stand on their merits or fall outright. The reader will notice in the second chapter, how, in following this method, the most accredited opinions are discarded without one's being able to reestablish them on any sort of foundation.

CHAPTER III It was still necessary to present in their order the different diseases of wheat that had up to this time been confused; to distinguish the less dangerous from those one should be the most disturbed about; to use, in naming them, the correct and characteristic terms; and to describe them with enough care to make it easy to recognize them. Somewhat has been added to chapter three, namely, a particular digression has been made on the disease of rye, which, generally, is known under the name of Horn or Ergot of wheat, and

there has been given there an explanation of the malformation of kernels attacked by this disease. The reader will note in the second part of this work that the facts agree with the explanation and that the sting of an insect is the real cause of ergot.

After a great number of observations on the nature of troubles peculiar to wheat, I recognized that one sought in vain the cause, whether in the meteors, or in the failure of maturity of the seed, or in the kind of manure with which one fertilizes the soil; and I believed, in fact, that I saw coming out of a matter so obscure some gleam of truth when I discovered the insects that live on wheat. These animals, always very small, and nearly imperceptible when they are young, belong to the class of Staphilinids [Staphilinidae]. I examined them with such attention and such secret joy as a new object can engender, and whence appeared to depend the issue of a great difficulty. I neglected nothing that might instruct me as to the precise point of their origin, the manner in which they were nourished, the kind of damage they were capable of making, and the relation this damage might bear to the diseases in whose origin I had so much interest.

In spite of the attachment that it is natural to have for new ideas, I never closed my eyes to what was not clear to them; the love of the truth made it difficult for me to censor my own system, and after having long regarded these insects as the possible causative agent of the major disease of wheat, I finally considered them such no longer and yielded to an hypothesis as hazardous as those hypotheses I had contested.

Perhaps I should have reduced somewhat the material of this fourth chapter, since it leads to nothing profitable on the origin of the ruined grain; but it occurred to me that certain others may be as forcibly impressed by the insect hypothesis as I was; I feared that this somewhat specious hypothesis would eventually have prevented them from correct conclusions had I not prepared them by revealing the futility of my observations and the little progress I have made, notwithstanding my persistence in following these insects, in a study of their behavior and in an effort to find the germ of the diseases of wheat within the refuse they may produce.

The correct method of preventing mankind from falling

prey to an illusory system lies perhaps less in methodically, and by the simple process of reasoning, revealing to them the spurious than by letting it be known how we ourselves have been attracted by it, have considered it in all its aspects, and, with something akin to regret, have finally abandoned it as incapable of proof. Further, one feels somewhat humiliated, as is always the case, when one has made a mistake and has fallen prey to an illusion; one enlightens others without wounding their pride; and still leaves them the honor of thinking that they had perhaps seen at first glance the entire fallacy of the system against which they had been warned.

In assailing in the second chapter, the various opinions that I have compared as to the cause of the diseases of grain, I did so only in support of my own general observations and from naturally drawn inferences. I soon realized that after devoting myself to helpful speculation it was still necessary to have recourse to experiments, to put my hand to the task, and reply to all by the facts disclosed before my eyes. I, therefore, made in the month of October, 1751, different sowings the details of which one may see at the end of the first part and the result in the second part of this dissertation.

After the wheat had attained a certain height, I recognized more than ever the extent to which tests, supervised and followed scrupulously, nullify false ideas, clarify present new views, and teach one to hazard nothing in judging the secret operations of nature. In the supplement of Part I will be seen the first fruits of my experiments; how I had to consider, with the greatest care the three principal diseases of wheat, to determine several characteristic facts with regard to each of them in particular; how I came to doubt, with good reason, that they had a common origin; was obliged to contend more than ever against the opinions I had challenged; and finally, to retrace my own steps, in giving up the thought that the insects already referred to were the inciting cause of any damage, from which the diseases of the crop had arisen. Moreover, the reader will find there the observations I made on the roots of diseased wheat, which were for a long time the object of my studies; I noted in them some changes; I suspected the insects of being the cause, and believed that I perceived some relation

between these changes and the diseases of the grain. So true it is that an idea with which one has been seized and that one has too inconsiderately accepted as a first ray of light, offers itself without ceasing to the mind, although it already may have been rejected, still pleases under a new aspect and seems to yield to the inclination of the imagination it has impressed. This supplement ends with a presentation of the cause of the disease of the kernels of wheat by showing in prospect the facts that the second experiments disclose.

The second part of this work is, in the main, to be considered only as a complex of experiments, a naïve exposition of the procedures followed, of the results that they gave me, of the deductions I drew from them, of the objections that I foresaw, of the answers I conceived. I do not at all, therefore, prejudice the reader in presenting to him this abridged part. It would be difficult for me to reduce it advantageously to some principal facts; it would appear dry, meager; the account I would make of it would have no grace, no expression; it would be better that one get no fixed idea of it before reaching it. The first experiments will prepare for and require those that follow; the dependent truths will follow in immediate sequence; the cause of the disease will be disclosed; the remedy will be anticipated with something like impatience; this cause, having itself a double origin, will become readily perceptible; the remedy will be wholly effective; and the reader, finding himself finally at the end, will be inclined to believe with me that the experiments were conducted with care.

DISSERTATION
ON THE DISEASE THAT RUINS THE GRAINS OF WHEAT

IN THE HEAD AND CONVERTS THEM INTO SMUT; And on the Methods of Preventing these Untoward Circumstances

THERE sometimes is complaint that the subjects of Academic prizes are of no interest to the public, and seem designed rather to exercise the talents of the man of the study than to procure a substantial benefit to society. Here is a subject that surely will not be confused with those of pure curiosity; it concerns the people generally; it impinges upon their interest directly and merits, in many respects, the attention of the best natural scientists. But, although from the viewpoint of natural history it offers something of interest and practical value, it presents many more difficulties than one might believe.

At first glance, to comprehend the nature of the particular disease of wheat in question does not seem to require arduous labor; it seems to be self-evident. But, when we consider the malady from the standpoint of its cause, it becomes more obscure; the disease is before us in its entirety: It has not been possible to set out from a known point of origin in order to follow the progress of the destruction of the kernel. The derangement of the organic parts of the plant has taken place insidiously; and, even though one recognizes it finally by certain external symptoms, it is then too late to disclose its cause.

After all, we need not be surprised if the diseases of plants have their obscurities. The admirable mechanism of plants is almost entirely revealed before our eyes. The thousand peculiarities that relate to their internal configuration, the action of their parts and their successive growth are unknown and will, according to every indication, remain so. One is still in doubt as to the exact movement of

the sap, which fills and nourishes them. The complete function of their parts is not at all perfectly known; the diseases without number to which they are subject have been but little studied. Why flatter ourselves on the easy recognition of the causes of their diseases, the application of suitable remedies, without having considered them carefully and without having made a special study of them?

The diseases of the human body have been, throughout the centuries, the subject of the most profound research. Many brilliant men have shed light on this essential part of natural philosophy; medicine to-day has attained a high degree of perfection; yet, how many things are unknown in the field of animal economy! How many diseases there are of which we fail to recognize the cause! How many remedies of which the success is doubtful!

Let us not hope, then, for an intimate and immediate acquaintance with the contingencies to which plants are subject. Let us accept as a fact that a clear understanding of the disease of wheat that has been made the object of this memoir requires much research and let us consider the degree to which it is veiled in uncertainty as to the provoking cause, even though the disease be everywhere common to wheat, and even though celebrated men have given it some attention.

Before giving an account of my work, and proposing new ideas as to the disease that ruins the grains of wheat in the head, and blackens them, it will be well to present the scope of this memoir, and to show, in the outline I have made of it, the exact procedure I followed in order to disclose accurate and fundamental facts.

First, I shall now recall as succinctly as possible, and with all that fidelity that is by right expected by a distinguished Society, all that has been said, and that is plausible, concerning the subject proposed by the Academy.

In the second place, I shall show by thorough proof, and through my own experiments, how the diseases to which one commonly attributes the destruction of the seed are attended by a multitude of obscurities, several of which appear inexplicable; and how the remedies employed, and in consequence of the assumed cause or causes, do not go to the seat of the trouble, at all.

Thirdly, in order better to characterize this destruction

and smutting of the seed, which I ought first of all to do, I shall present in detail the diseases of wheat that have seemed always to have been confused under the too general names of smut, blight, black wheat, etc. I shall differentiate them by certain peculiar characteristics; I shall at times show a sort of liaison between them by attributing to them the probability that they may have had a common origin; and I shall compare with these same diseases of wheat those that are slightly similar and known to occur on other plants.

Fourthly and finally, I shall give the details of my own observations on kernels of wheat ruined in the head and reduced to a blackish dust. They will be designated in due course under the name of carious, smutted kernels. I shall propose some conjectures on the primary cause of this disease. I shall advance nothing that I have not seen with my own eyes, and that, too, several times. I know by experience how much one runs the risk of being deceived in matters with which he is concerned when he does not speak in accord with those who have considered them, or when one adopts too easily the discourse of even the most intelligent farmers. I shall not defend the doubtful in my observations; I shall lay stress upon whatever will have appeared to me to be conclusive; and I shall take care to offer nothing as certain that I have not consistently noticed in the different Cantons.

CHAPTER ONE

FIRST PART

Diverse opinions on the disease that ruins the grains of wheat, and smuts them. Means that have been believed capable of preventing these unfortunate results.

The newspapers are commonly enough the vehicle employed in demanding light on some interesting point or to announce a useful discovery. The cause of the corruption of the grains of wheat in the head, and the remedies one may apply deserve certainly to be studied with care. This subject is of a nature designed to impress the minds the least qualified to inquire and seems to require that every citizen give it his attention. Therefore, the Journal of Verdun, which is quite generally distributed, has several times been appealed to for information on the cause of the wheat rot and to instruct the public on observations made thereon.

Let us pass in rapid review the references in this periodi-

cal wherein the subject in question has been touched before we pause to consider in more or less detail the statements of recognized contributors. These references, it is true, furnish few facts; with difficulty some traces of light penetrate the mass of snap judgments and chance observations; but, meager as they are, I believe they should be assembled: the record of errors, moreover, has some advantages; it presents to us a thousand mistaken ideas, and serves to introduce the truth.

One finds in this journal a letter that describes quite fully the disease of wheat with which it treats. An error, not at this moment to be disclosed, has, however, crept into the account. Journal de Verdun 1730. January, page 31.

Subsequently, the manner of treating the seed is indicated, which deserves some attention. If one doubts that the recommended treatment is as beneficial as we desire it to be, it at least is a convenient and perfect method of washing the seed. Change of seed is prescribed here as a precaution often taken by the farmers of Picardy, and one the usefulness of which they have recognized. 1739. October, page 244.

In 1739, a great part of the wheat in the Electorate of Chattellerault, was attacked by smut. The author, who noticed it, carefully observed the symptoms of the disease in their every aspect. The dangerous mists, which fall on the wheat when it is in flower, appeared to him to be the only cause of the disease; he saw no danger in sowing smutted seed; and every method of seed treatment seemed to him useless.

The system changes. The farmers of the Caux country, persuaded that the same seed degenerates in soil of the same kind, and that the disease of the grain may result through the use of seed that has run out, have taken care to obtain their seed from a poorer country than their own, and above all to avoid sowing contaminated seed. 1739. December, page 437.

Here is an opinion quite different from what I have presented. *Idem.* Some say wheat is ruined by the moisture absorbed by the grain put aside in the barn for seed. A sovereign remedy, the details of which have not been made known, has been reported, a remedy that invigorates and increases the resultant plant growth, apart from any other amendment or salutary agent. Some fields of wheat, subjected to this treatment, have become very excellent,

though sown in the month of March, and have produced at least as much grain as fall-sown fields.

This treatment, if repeated experiments establish the surprising effect, should be a great boon after a winter as rigorous as that of 1709. It would assure the success of second sowings, without which treatment all would be futile, under the temperature and seasonal conditions that so commonly prevail in France. M. Duhamel demonstrated this by sowing wheat in February and May; he did not succeed. I made the same test in March, with as little success.

Mén. de l'Acad. des Scienc. 1746. Pages 76 and 77.

Could it not probably have been due to particular circumstances, in combination with several causes favorable to the growth of wheat, but rarely experienced in the months of February and March, rather than to the kind of seed treatment, that the success of the March seeding was due? The order of the seasons does not permit the sowing of wheat in Canada before spring, and even then not before the end of April, in regions in the neighborhood of Quebec. Wheat there, however, comes up, ripens, and brings its heads to full maturity in the space of some months; the harvest ordinarily occurs there toward the end of the month of August.

Mém. de l'Acad. des Scienc. 1746. Pages 92 and 93.

The author of the reported opinion again asserts his adherence to it, and adds that even the imperfect maturity of the seed may be a cause of taint. The remedy presented in the foregoing prevents, says he, this latter trouble, because it is a preservative of the seed against humidity. It will be seen in due course how this author is deceived concerning the real effect of his preparation; but, without knowing the cause of the disease, he observed useful precautions.

1740. Février p. 14.

Page 117. In the same journal one is advised to use arsenic for the treatment of the seed, and change of seed is spoken of as being helpful.

Idem. Different memoirs are cited there on the disease of wheat, from the summary of which it appears that one of the better methods to which one may resort in order to avoid the destruction of the seed is to abstain always from sowing a given kind of seed in the same soil, to sow only healthy, perfectly dry, and mature seed.

Idem. Farther along, one is instructed against the use of arsenic

in the preparation of the seed, and the serious effects that have followed its use are described.

Common opinion appears here in a new phase and supported by some plausible reasoning. A physician, moreover, very well informed, sees no advantage to be expected from the different seed treatments other than a more prompt germination. The cause of the disease lies, according to him, outside of the grain. It is a purely ethereal, volatile oil and, moreover, viscous, which falls on the plant, even in fair weather; he calls it *Enmiélure*. This material, activated by the sun, becomes instilled into the substance of the most delicate bodies. It alters the still tender fruits, the leaves, and sometimes the young shoots of trees. The kernels of wheat, which it penetrated while they were still in the milk, are arrested in their development, become diseased, and, one might say, gangrenous. The different stages of growth in which the grain may be when this material penetrates them result in different manifestations of the disease. From the observations of this physician it is obvious that the *Ebrune*, the term used by him to designate the dust of smutty wheat, is a disease one may try in vain to remedy.

1741. July, page 22.

Another word concerning a new opinion and I close these extracts. The author assumes this attitude: He cites the experiment and assumes that sheep manure, spread on soil that has just been seeded, is the cause of the bad condition of the seed.

1751. May, page 365.

This opinion is promptly attacked. It is tested by experiment after experiment, and it is concluded that the heat of the manure is not at all the source of the disease with which we are concerned.

1751. September, page 205.

The authors of different treatises on plants, or on agriculture in general, have considered the particular disease of wheat we are examining. They have sought the cause of it, they have stated their opinions; but always they have left the subject veiled in almost as much obscurity as it was in before they had reflected upon it.

Without referring back to authors of remotest antiquity, it will suffice here to cite Jean Bauhin, celebrated botanist of the last century, who ignores nothing in his writings and who reports the opinion of Theophrastus and of Pliny on the origin of the corruption of wheat, after having presented what he himself had thought concerning it.

Hist. Plan. tom, 2, page 418.

He said that, ordinarily, one could perceive the contamination of the grain when the head emerged from the stem [boot]. He attributes it to the action of the sun, the burning rays of which fall at intervals on the head that an abundant rain has penetrated; he assumes that the leaves and stems do not die at all, but that the kernels change into a blackish and sooty powder.[1] This botanist subsequently reports on the origin of these black kernels the opinion of Theophrastus, which differs little from his, and seems to be the basis from which he drew his.

Theophrastus regards as very essential for the health of wheat the situation of the piece of ground where it grows; he assumes that, when the wheat is in low places and little buffeted by winds, the rain water lingers in the heads and becomes a source of injury as soon as the sun strikes them.[2]

Theophrastus adds that wheat is especially damaged during the full moon and gives a reason for it, the fallacy of which none of his contemporaries knew; that is, that the moon may heat substances and occasion, by its heat, injury of the heads during the night, just as he assumes that the sun may contribute to the same effect during the day.[3]

Jean Bauhin, after having reported the opinion of Pliny, who said nothing decisive on the point under discussion, finishes by designating the months when wheat begins to show impairment and returns anew to the cause to which he attributes the smut of the grain.[4]

The celebrated Mr. Wolf, in his book[5] on the multiplication of plants, devotes himself at great length to the cause of the destruction of wheat: the manner in which he depicts

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[1] *Ustilago, ubi in spicam abit culmus, apparere solita, si pluvia madens, incert folis sentiat fervidiores radios; culmo foliove neutiquam degenerante, sed granis in fuscum, fuliginosum pulvisculum commutatis.* Theophrastus.

[2] *Nec segetum situs (according to the translation of Gaza) naturaque parum refert: tractus enim excetsi perflatique rubiginem nullam aut minimam sentiunt; concavi & flatibus nullis expositi contra frequentissimam habent . . . spicam inclinari utile est ut imbres decidant, nec rores excipi possint . . . Segetes vento expositae minus aerugine infestantur . . . Elatus enim humorem dejicit, & sol, paululùm immoratus, nullam putredinem conficit; sed, priusquàm emarcescat humor, totus assiccatur atque absumitur: aerugo autem quaedam putredo est; nec putridum esse quicquam potest fine alieno calore.*

[3] *Ratio in calore est quo etiam noctu luna putrefacere potest.*

[4] *Caeterum nascitur ustilago Aprili & Maio mensibus, cùm coeli tempestas crebrò inconstans aliàs nimbosa aliàs serena fervens fuerit: tunc enim spicae humore madidae à solis fervore corrumpuntur, & quodam modo, uti aiunt, aduruntur.*

[5] This book is written in German: the Actes de Leipsic rendered the title in the following words: *Vera causa multiplicationis Frumenti admirandae:* and provided an abstract of it in 1718, page 178. Manget reports it in its entirety, Vol. 4, page 644.

it is, nevertheless, a little confused and he is obscure as to the kind of disease he has in mind. He does not state, which is nevertheless essential, whether the grains of wheat, in becoming internally converted into a black dust, retain their epidermis and their essential shape; he seems only to wish to speak of a malady to which wheat, barley, oats, etc., are exposed by employing a general term, which amounts to that of blast. He does not distinguish, as we shall judge later on in this memoir, the particular disease with which he is concerned from the one that affects the head as a whole. We shall speak of it ourselves in the article where it shall be a question of characterizing the principal diseases to which wheat is subject.

An appended extract from the chapter by Mr. Wolf, on the smut of wheat, would fatigue the reader; I am limiting myself to observing here that this savant, after having discussed the opinion of different authors on the subject in question, concludes by regarding as laid bare to its foundations the causes to which it is customary to refer the smut trouble and finally gives an exact account of his own observations. They led him to construe the cause of smut as a sort of monstrosity, a faulty conformation of the grain. They have led him to believe that the nourishing sap, which moves in plants, becomes contaminated in the vessels of the smutted kernels; because these vessels, being of an aspect contrary to nature, stop within themselves the course of the sap, and thereby become a cause of disease.

The author of the *Spectacle of Nature*, in an interesting detail on seed, particularly wheat, on its germination, on its growth, and on the practice of different sections, has not failed to say a word on the disease to which it [wheat] is subject and, like a careful observer, all but explained the manner of judging what he examined.

"Everyone knows," says he, "that rust[6] appears after an extremely light rain and is followed by a burning sun; when these raindrops rest on the stem, they become there so many little burning glasses, which burn, depress, and blacken the stem at so many points." Tom. 2, page 314.

[6] One sees here how necessary it is to establish the meaning of terms one employs in describing the diseases of wheat. M. Pluche seemed to mean by the word *Nielle* only the alteration of the stem, while others understood it to mean the destruction of the kernel.

"Drizzling rain does not prevent wheat from growing as does black rust; but it converts the kernels into a black and putrid powder. In examining, last year, several blasted kernels, I found them without a germ, and I perceived almost always, either laterally or underneath each kernel, the two flowers, which, having failed to mature, to eject their pollen, had remained there without furnishing the kernel the principle that develops and perfects the starchy content with which the grain is filled. Such is the disease; but who will furnish us the remedy?"

Page 211. M. Duhamel, in his excellent treatise on land cultivation, tells of different diseases to which wheat is subject, and presents them in a very methodical order. He first distinguishes under the expressive names of "rusted wheat," "rickety wheat," "scalded and shriveled wheat," and "wheat invaded by insects," the diseases known in England by the name of wheat blights. He assigns them to separate classes in setting forth their more or less varied consequent effects; he traces only the causes of those diseases that appear the most natural and passes on to the examination of

Page 224. the disease that is the subject of this dissertation. He considers it, apart, as more important than the others and designates it under the name of black, or smutted wheat; let us consider briefly what he has written about it.

After having described this diseased wheat, such as we have already characterized, he says that black wheat does not germinate. He assures us, contrary to the opinion of Mr. Tull, that the sound kernel, the little brush end of which has been blackened by smut dust, has in itself no tendency to produce smutted wheat. He avows that one does not yet know the cause of this disease. He appears to be convinced that smut is to be feared if much cold rain follows during development of the head. He sets forth the opinion of Mr. Tull, who thinks it is not the water that falls on the stem, the leaves, and the head that damages the kernel, but that it is too much soil moisture. He cites the following experiment performed by Mr. Tull in order to support his opinion: We shall soon see the justice in M. Duhamel's wishing that it might be repeated.

Page 227. "Mr. Tull dug up some wheat plants in a field and planted them between two casements of his room, in a vessel full of water, or at least in some very wet soil. These

wheat plants produced some heads, but all of the kernels of these heads were black and smutty, although in the field from which he had taken the plants there was no smutted wheat. Evident proof, says Mr. Tull, that this malady resulted from the excessive moisture present in the soil, and not from that which wets the heads and stem; for no water had fallen on the wheat that was between the casements of his room."

M. Duhamel, far from regarding this experiment as conclusive, noticed, on the contrary, that he sometimes found as much smutted wheat in the highest parts of sown fields, where there was little moisture, as in the lowest of these same fields, where the water was obliged to stand. This learned Academician finished by referring to Mr. Tull's method of treating the seed to protect it against smut. An accident, concerning which it will be necessary to read the author's account, furnished the idea for it, a treatment that consists in sprinkling the wheat with a strong brine of sea salt, by dusting with well-pulverized quick lime, and by stirring well afterwards in order that all the kernels be equally exposed to the mixture. Mr. Tull, on the testimony of a farmer in whom he had confidence, speaks also of a change in seed as being helpful. Besides, the English naturalist assures us that since he began cultivating the soil in accordance with the new method he has had no smut in his wheat. M. Duhamel, who has followed him with this kind of practice, as one knows, has not been so fortunate in this matter. He says that this method (otherwise excellent) does not guarantee wheat against smutting; and that one *arpent*, cultivated according to the new rules, contains almost as much smut as another *arpent* in the culture of which these principles have been omitted.

Expér. relat. au Traité de la cult. des Terres Page 20.

In the splendid review of the memoirs presented to the Academy of Sciences by various savants, there is one very remarkable memoir on the growth of plants on other media than the soil. M. Bonnet, of the Royal Society of London, Correspondent of the Academy and author of this memoir, expounds on the cause of wheat blast. He believes we should attribute it to cold dews, which the first rays of the sun activate. He assumes that wheat plants growing in elevated places, like the mountains, are less subject to this disease than are those of the level country, and especially less so than those in low and moist places. He states that

Mém. de Math. & de Phis. tom. I. 1750.

Page 431.

one observes much more rarely smutted kernels on bearded than on beardless wheat. He gives as a reason for this the fact that the beards doubtless shield the kernels from the dew and prevent it from clinging to them. He brings to his opinion everything that he believes able to favor it, but asserts, nevertheless, that his observations should not be regarded as conclusive, since he has not made a subsequent study of the particular disease of wheat with which he was concerned.

CHAPTER II

Observations, certain facts in the light of which one examines the opinions presented in the preceding chapter

BEFORE adequately summarizing the different memoirs that I have already presented for analysis, before destroying the majority of the conceptions that have been advanced, in order to square everything with the opinion once adopted, it appears absolutely necessary to consider the too-much neglected practice of amassing a certain number of well-established facts and of speaking only in accord with exact observations. Although some of the observations that I have presented had already been made, I have, nevertheless, verified them with care; one may regard them all as the fruit of scrupulous study. I shall regard them, moreover, as so many principles to which I shall always refer, whether to judge of what will have been said by various authors, or to adjust and fortify my own ideas.

FIRST OBSERVATION

The black and blighted heads borne by the different culms, but deriving their roots from one and the same seed, are, ordinarily, all black and spoiled.

SECOND OBSERVATION

One and the same seed produces sometimes good and bad heads; a given head bears healthy kernels and blasted kernels; a portion of the head is sometimes ruined and another part is not: a bad kernel sometimes is found between two good ones, or a healthy kernel between two bad ones.

THIRD OBSERVATION

In the glumes that enclose the black kernels one finds consistently the three stamens; they have adhered to the kernel; their tops extend one-half to two-thirds the height of the kernel: They sometimes rise to its full height; but they rarely exceed the height of the kernel.

FOURTH OBSERVATION

One often finds blasted heads even in the sheath, although the sheath may be green, completely closed and leading one to suspect no contamination.

FIFTH OBSERVATION

Bearded wheat, as well as beardless, is subject to blast.

SIXTH OBSERVATION

Kernels of rye, although very delicate in their early development and subject to a particular kind of disease, are never converted into a blackish dust as are kernels of wheat.

SEVENTH OBSERVATION

A wheat plant, purposely placed in a constantly impoverished soil, produced many heads in which not a single kernel was found that was black and ruined.

In the first memoir, taken from the Journal of Verdun, the author offers nothing concerning the origin of the disease of the kernel; he is content merely with describing it. The author of this memoir deceives himself, according to the third observation, when he states that the blasted heads blossom like the healthy ones. The reply to this memoir adds nothing as to the cause of the disease; it presents only some palliatives such as the selection of seed, a preparation comprising various ingredients, and a change of seed. There will be no question for the moment as to the advantage or the usefulness of seed treatment prior to sowing: We shall speak on that later, along with the custom of changing the seed, practiced by some growers; and we shall see how one and the other precaution do not go directly to the source of the trouble, or at least remedy it only in part, and seem founded on some known benefits.

In attributing the cause of wheat blast to the mists that obtain while the wheat is in bloom, and that enter between the glumes when they are half open to permit emergence of the stamens, one apparently is deceived, according to the first, second, third, fourth, and sixth observations. Deceived according to the first, because it would be singular that several heads, all derived from the same seed, should be ordinarily all spoiled by the mists, while other heads belonging to another seed, showed none at all, although the

stems of these latter were often mingled with those of the first, and grew in such manner that the healthy heads were alongside the diseased ones. According to the second observation, one is apparently deceived. How, in fact, can one explain by the influence of mists this mixture of good and bad kernels, which one finds sometimes in the same head? The third also is wrong because stamens, never rising above the kernel, are unable to open the glumes and give access to the mist. According to the fourth, we are deceived because the head is often blasted before the sheath opens. Finally, in attributing this malady to the malignity of mists that intercept a scarcely perceptible, tender, delicate grain, how can one adjust this to the sixth observation? Are not the kernels of rye in their incipiency extremely delicate? Are not mists more common and more dangerous while rye is in bloom than when wheat is in that condition? Rye, however, is never subject to the effect of mists or, at least, allows nothing to bring about the effect that is attributed to it in wheat.

In *Pays de Caux* the farmers attribute the cause of wheat blast to seed that has never been changed, or to smutty seed of similar history. Consequently, without concerning themselves with any external cause, they consider the seed placed in the ground as becoming the one source of the disease because of its internal imperfection or because of some exterior defect, even before it produced a crop. But, it will be said, in supposing the kernel vitiated in its beginning, or in a purely accessory manner, how, following the second observation, can it sometimes produce good and bad heads, good and bad kernels? This objection is without doubt a formidable one, and I agree that I was at first puzzled; but I have finally recognized, through some carefully conducted experiments, and of which I shall give the details, that contaminated wheat kernels produced smut in abundance and produced it with the inconsistencies that give occasion for the objection.

The author of the following opinion, in giving as the cause of the disease lack of maturity or dryness of the seed, and in adhering to the custom by which only well-ripened and well-dried seed is employed, will not explain thereby the origin of a disease which, carefully examined, is peculiar to wheat, to rye grass, and perhaps to some other plant. The

cause that he indicates is general: if it were the real source of the trouble, it would be evident from time to time in all the different kinds of grain and would produce therein the effect that the author says results from it. No more attention is given to the ripening and curing of rye than is given in the case of wheat. The same farmer who harvests sometimes badly damaged wheat always harvests rye that is not, or that is not damaged in the same way. Would this farmer fail to give attention to the most valuable of all the cereals; would he neglect only the seed of wheat? We would prefer to state that the damp and imperfectly matured kernels will perhaps drop prematurely, or become diseased, but they will not show the peculiar effect that we have under consideration.

The physician whose system we have presented, will have some difficulty in reconciling the majority of our observations. In attributing the disease of smutted grain to an ethereal, volatile, viscous oil, which the sun activates, he gets himself into a multitude of difficulties the outcome of which I have not been able to determine. Several of the reasons I have given in the foregoing, in order to exclude mists as a primary cause, are equally opposed to his opinion and should be regarded as rendering it essentially insupportable, if they be sufficiently sound to warrant their adoption.

Those who assert that wheat, sown in a soil immediately after having been spread with fresh sheep manure, runs the risk of becoming smutted are apparently deceived, according to an experiment that I shall have occasion to report. They attribute the disease to an alleged excessive heat, which affects and alters the kernel. The reference I have made to the subject of contaminated wheat kernels that produce a great quantity of smut and, sometimes, transmit it only to certain heads of a given stool, impels me to offer an idea that I, at first, believed capable of refuting the opinion I here attack, that a kernel of wheat, originally defective, ought either to rot in the soil without germinating or produce only a languishing plant or transmit generally and uniformly to all the heads it produces the more or less marked imperfection of which it is the source. But I shall ever be obliged to maintain, to the utter ruin of this opinion that, in order to damage the grain, a much higher degree of heat would be necessary than that supposed, in fact, to be pe-

culiar to sheep manure from the stables, but that soon had cooled on the soil because of the care with which it was spread in order to break it up into small portions; and that, even in the case where this burning fertilizer injured the grain by excessive heat, it was, according to my experiment, an injury that resulted in the complete desiccation of the germ and its consequent incapacity for further growth, rather than in inducing it to produce wheat plants from which rise sometimes as many as 8, 10, 15 very vigorous and laden stems, which, although contaminated, are very long and well filled with grain. In support of this reasoning I shall further add that the grain is not damaged by the common treatment with limewater, the heat of which, when one pours it on the grain, is much higher than that that would obtain even with finely divided manure.

The opinion of Theophrastus and that of Jean Bauhin no longer agree with several of our observations. They attribute the blight of the grain to the action of the sun, which strikes the head actively soon after an abundant rainfall has penetrated it, and they assume, in consequence, that the elevated and wind-beaten places are less likely to bear smutty wheat than are the low places, where the slightly disturbed and protected heads hold the moisture a long time. In the foregoing we stated that M. Duhamel had noted that the exposed elevated parts of fields sown to wheat contained as much smutty wheat as the lowest places of these same fields. First presumption against the opinion of those two authors. Otherwise, how would it be possible for him to return to the first, second, and sixth of our observations? Does the water activated by the sun act chiefly only on the heads that arise from the same seed? Is it to be presumed that rain penetrates extensively the heads that are completely closed within the sheath? Would rye, the kernels of which have never been observed reduced to a dust be more capable of resisting the joint action of rain and sun, considered so dangerous for wheat?

Mr. Wolf is persuaded that the sap, arrested in its circulation, this nourishing juice, stagnating, so to speak, in the false kernels that are like so many monstrosities, becomes there a source of corruption. In reporting the opinion of this scholar, we have said that he had not distinguished the two kinds of diseases of wheat in which the substance of

the seed is changed into a black dust: we shall soon supply this distinction: we shall give the details of a third disease, which he appears not to have known and of which I have not observed that any one has thus far spoken. The opinion of Mr. Wolf requires special discussion, and it will be better to await the exact knowledge of the three major diseases of wheat in order to discuss how these diseases have been distinguished one from the other. Then one will be in a position to understand, if one explains sufficiently, how, by the failure of circulation of the sap in the false structures and the desiccation of the malformed kernels of wheat that, according to every appearance, Mr. Wolf has ignored, it is absolutely impossible to apply the same reasoning to the wheat grains reduced to a dust concerning which he expects to speak.

The behavior of the affected stamens appears to be, according to M. Pluche, the cause of the blasting of the seed. It already has been seen that this opinion squares with our third observation. This author errs in stating that there are only two flowers, or stamens, beside the kernel. There are consistently three of them; and it is contrary to the natural order when one finds but two. Moreover, we shall show the agreement of this idea, resulting from an inspection of the kernel, with the ideas we shall present after a subsequent examination and to which we have for a long time adhered, in believing them more valid than they are.

M. Duhamel, author of the treatise on soil cultivation, who, unfortunately for natural history, did not study the cause of smutty wheat as he has applied himself with success to several interesting subjects, and especially to the perfection of agriculture, takes no particular interest in the cause of seed blight. It would seem that the cold rains that follow when the head is formed should, in fact, cause him to apprehend this unfortunate circumstance; but this opinion reverts to others I have presented in the foregoing and offers difficulties that are keenly felt to become greater in the light of our several observations.

Mr. Tull, based on the experiment that we have reported, believes that excessive soil moisture occasions the decay of the seed, and asserts that he has had no smutty wheat since he employed his new method, and that he has been careful to provide some furrows designed to facilitate the drainage

of the water. In order to cast considerable doubt on the validity of what Mr. Tull believes he observed, we would need only to cite M. Duhamel, who never noticed more smutty wheat in low, moist places than in elevated places, and who, in the experiment he conducted according to the principles of the English savant, was unable to guarantee his wheat against this malady. But I shall report here the experiment that I, myself, made in order to verify that of Mr. Tull; to me it appears decisive; I have felt that I should here give it in detail.

In the month of May I dug up a wheat plant, composed of several distinct tillers; it was put in an earthenware pot filled with common garden soil; this pot, the shape of which was that of an inverted bell, was 9 inches deep; its principal diameter, taken at ½ inch from the lip, was 11½ inches; and the minimum diameter, taken at 1 inch from the bottom, was 7¼ inches. The wheat was watered 4 times every day, regularly, until the beginning of August; each sprinkling amounted to a little more than ½ pint of water, so that the 4 waterings, taken together, formed about 2½ pints.

It is quite properly assumed that this plant, which was in a continually drenched soil and exposed to the south, should have grown with vigor, as, in fact, it did, in producing as many as 105 stems, of which one-third bore heads laden with grain, one-third bore some heads only partly filled, and the other third bore none save empty heads. But in this great number of heads I found none that contained diseased kernels, none whose kernels had been converted into a blackish dust; several heads were, to be sure, poor; a great number of kernels were shrunken; the majority of the stems were short and weak; but it was necessary to attribute these shortcomings to the too great abundance of stems within a very limited space, and to conclude, moreover, that, since so many heads, either in a state of perfection, or in a state of mediocrity, or in a condition of emptiness, contained no smut, the excessive soil moisture is not the real cause of the trouble. I should make note here in conclusion of my experiment that the stems and leaves of this wheat plant, nourished with water, remained quite green during the entire time that the wheat was watered, but by the beginning of August the water having been suddenly discontinued in order that the wheat might ripen perfectly, and

mature, the stems and leaves dried in a few days; and it is, according to every appearance, to this discontinuance that one should attribute the blighting of several kernels.

It remains for me to say only a word concerning the opinion of M. Bonnet on the origin of smutty wheat. He attributes it, with as little basis in fact as does Mr. Tull, to the humidity of low places, and believes that elevated places are less subject to it; he assumes, moreover, that the cold dews, which the heads have imbibed at the moment the sun flashes its first rays upon them, occasion the spoiling of the grain. M. Bonnet believes him, according to a remark that he expressed in due course and that deserves some attention. "It is observed," says he, "that diseased kernels occur much more rarely in bearded wheat than in beardless; the reason being, without doubt, that the beards hold the dew apart from the grain and prevent it from coming on contact with it." I shall be able, however, to assert here that, in the examination I have made of an infinite number of diseased heads, I have never been able to perceive that the beardless heads were afflicted more commonly than were the bearded ones. I have under observation a considerable quantity of heads, which I drew at random from several bundles collected in different cantons: In assembling them, I had in view only my special observations, and not the remark of M. Bonnet of which I was then ignorant. Since reading his memoir, where this that relates to our subject is only incidental and does in no way detract from the real value of the work, I have examined my little bundles of smutted wheat, and I have found in them at least as many bearded as beardless heads.

CHAPTER III

Differentiation of the Diseases of Wheat

ARTICLE I.

Description of the less dangerous Diseases of Wheat

I HAVE already stated in the course of this memoir, and the reader ought, himself, to have noticed it, that the authors, whose opinions on the causes of the disease of grain I have reported nearly always confuse, under a general name, all of the diseases to which wheat is subject, and assign to them a common origin. I have observed, however, that M. Duhamel distinguished with care several maladies to which wheat is exposed, and that he explained the different causes of them.

Rusted Wheat

He designated as *rusted wheat* that wheat the stems and leaves of which are covered with a reddish substance of the same color as the rust of iron. This substance, which becomes a very fine dust, is not absolutely adherent to the stem or to the leaves; it adheres to the hands on the slightest touch of the plant on which one has observed this reddish orange dust. The parts of the plant that are attacked by rust almost cease to grow; this disease, which causes sometimes a considerable crop loss, is commonly attributed to light mists, followed by bright sunshine.

This singular disease, rust,[1] requires an explanation. It

[1] The Romans recognized among several rural deities a god under the name of *Robigo* or *Robigus*. They celebrated, in his honor, feasts called *Robigalia* or *Rubigalia* and offered him sacrifices the seventh before the Calends of May, on the assumption that wherever they were he had the power to preserve their wheat from rust.

Nothing proves better how much of obscurity ruled at this time concerning this disease, and to what degree it has been confused with other untoward circumstances to which wheat is subject, than the terms employed by the ancient Latin authors and, after them, such moderns as Jean Bauhin and Longius in order to designate these diseases; terms that do not have the same meaning, that portray different symptoms, and that, nevertheless, these authors employ indifferently. *Ustilago, uredo, urigo,* which properly signify *burning,* and are rendered by the Greek word *εμπνεισμος,* often take the place of the terms *aerugo, rubigo, robigo,* which properly signify *rust* and are expressed by the Greek authors by the word *ερυσιρω.* Now rust of wheat certainly is not the same disease as two others of which we shall soon speak under the name of *smutted wheat,* and under the name of *carious wheat.* Trévoux's

doubtless would not be astonishing if certain mists, which one might conceive as being charged with nitrous and mordicant particles, become attached to the stem and the still young, delicate leaves of wheat, and changed them appreciably. But should one adopt the opinion of those who assume that this reddish substance, designated in some places under the name of honeydew, is absolutely foreign to the plant on which it is observed, and that it has been deposited there by mists? Should we not assume rather that the acrid particles of these mists have reacted severely on the stems and leaves, rather than that they have broken the tissue in some places and have caused the extrusion of a soft and oily sap, which has dried little by little, and become converted into an orange red powder. I have examined with a large hand lens several wheat plants whose stems and leaves were laden with rust; I have clearly observed that wherever the orange red powder was there also were some little cracks, and that the epidermis, either of the stems or of the leaves, was fractured here and there. I have observed this sap, reduced to a reddish powder, come out from these little openings, above which could still sometimes be seen the small portion of the epidermis, which, in tearing apart, imperfectly covers these little openings, and which one may compare roughly to that thin skin sometimes remaining on one's finger after an obliquely made cut.

In thus asserting that a thick and oleaginous sap escapes from certain plants, I am announcing nothing that experiment does not confirm, and that has not been previously noted by some competent observer. M. Reneaume, in a memoir devoted to the consideration of the nutrient sap of plants, expostulates at great length on sap exudation. He recalls that much already was known concerning the manna of Calabria, formerly believed to fall from Heaven, which is

Mém. de l'Acad. des Scienc. 1707. Page 276.

Deut. Ch. 28. v. 22. Les Rois, Liv. 3. Ch. 8. v. 37. בירקון Ustilago. ירקון Rubigo.

Dictionary, on the article *Rubigo*, also confuses rust with the disease of the kernels that are reduced to black powder or smut. These untoward conditions, however, are well distinguished in Holy Writ. Among the great number of evils with which Moses menaced the Israelites, if they did not obey the Commandments, he placed rust and the blast of wheat. The Hebrew word, which I translate here by blast, is rendered in the Vulgate by these words: *aere corrupto*. But one gives to it more commonly the sense I have here attached to it. The Septuagint has translated this same word by the term ἐνπυρισμός in one place in the celebrated prayer of Solomon where this religious prince supplicates the Lord to be touched by the cries of his people when famine, pestilence, the blast (of the wheat), the mildew [rust], and the ravages of the different insects will come to afflict them. Solomon knew from cedar to hyssop and, without doubt, was not ignorant of the various diseases to which plants are exposed.

nothing other than the extruded sap of the wild ash. He tells of a kind of manna extruded by the walnuts of the Dauphin, and of which the too abundant extravasation ordinarily causes these trees to die. Mr. Muschenbroek says that "after a very hot day, not only the fine and aqueous particles of the plants are found in great motion, but also the thick and oleaginous particles, which, being extruded from the excretory vessels of the leaves, without being able to return, are unable to evaporate into the air and, as a consequence, are obliged to remain on the surface of the leaves, where they form a thick material, like honey." One may see in the contribution by Mr. Muschenbroek that this opinion is that of a great number of observers and that it is based on some experiments that we are unable to accept.

Essai de Phis. Page 759.

Those who assume that rust is a deposit made by the mists alone can not explain satisfactorily why a certain stem is covered with this reddish substance, while none can be observed on a neighboring stem; for, admitting this to be true, the influence of mists should necessarily exert a general effect upon the more of less extended areas covered by them; and also because the stems and leaves should be affected quite uniformly by the fall of a thick, unctuous, and, without doubt equally dispersed material. Rather do they account for this unusual condition, i.e., of rusted wheat plants alongside of others that are not rusted, by stating that certain stems more delicate than others, and overtaken in particular condition, may be altered by mists, or such other thing as one might never suspect; that they may lose a part of their unctuous sap through points of injury, and, consequently, produce only poor and shriveled grain: while other more vigorous stems will resist the impact of the dangerous mists, will retain all their sap, and will bear excellent grain. That that happens in the order of animal economy seems to justify this explanation. It is observed, in fact, that there sometimes obtains in the air a malignity, that this contagious air penetrates our very bodies, that it occasions general, unfortunate circumstances; that many people, however, are immune from it, without being able to give any reason other than the particular condition in which they find themselves, and the nature of their temperament.

This detail on the subject of rust was necessary in order to show how they deceive themselves who regard it as the primary cause of the disease of wheat, and as an external agent that alters the wheat, while, within the wheat plant, it is the result of a disease the cause of which is not well known. This detail may serve also to enlighten those who believe that rust and this farinaceous dust that one notices on certain plants are accumulations of eggs deposited there by certain insects, and from which emerge a numerous family that is very baneful to plants. In accepting the opinion that I present to them, they will realize that rust, honeydew, powdery dew, and those fatty materials that one sometimes notices on the leaves of gramineous plants, of shrubs, and of trees, are the exuded juices, which gradually drying, become converted into an impalpable dust on certain plants, and change in color according to the species of the plant and the nature of the trees on which they are found. The exuded sap is yellow on broad beans, reddish on gramineous plants, and a pale green on the leaves of the plum.

Blasted wheat.

I return now to the distinction made by M. Duhamel between the less important diseases of wheat. He regards as *blasted* those plants whose heads, instead of being filled with good grain throughout their length, are absolutely empty to their tips, or contain only small kernels, which pass through the sieves and in which there is no flour. He believes that this condition may be occasioned by cold rains that sometimes fall at a time when wheat is in flower and that become an obstacle to fecundation. He expatiates also on the idea that lightning may cause the wheat to blight and speaks of extraordinary frosts that injure the head, emerging from the boot, either destroying generally all the grain, or causing only occasional damage.

Scalded and shriveled wheats.

Under the name of *scalded* and *shriveled* wheat plants M. Duhamel includes those that ripen without filling, that bear only very pinched, withered kernels, which contain less than half the flour found in normal kernels. This imperfection is occasioned, in his opinion, either by the meager nourishment that the heads receive when the wheat is turning and when the stem, more or less bent and suffering from a sort of strangulation, does not communicate enough material to the grain, which ripens without filling; or by the

great heat that follows after the wheat has been provided with moisture, which dries the straw promptly and kills the grain before the farinaceous sap has been abundantly transported to it. The experiment I performed by growing a stool in very moist soil exposed fully to the sun, the great number of shriveled grains I found in the heads that developed, prove the correctness of M. Duhamel in regarding this imperfection of the grain as a consequence of heat and excessive moisture.

The two different causes of shriveled wheat that I am reporting are, according to this learned Academician, not the only ones to which he attributes this malady; he assures us that insects, too, sometimes occasion it. I would have wished, and it will be realized at the end of this memoir what the motive of it was. I would have, I say, wished that M. Duhamel had entered into some detail on the nature of the insects that prey upon wheat and that he had indicated the manner of his examining carefully the wheat that was studied for the real causes of the trouble. But he is contented with speaking of the insects without depicting and without really explaining how they materially damage wheat: the following is what he says.

"It happens frequently in cold countries that there are some insects that puncture the stems of wheat before the grain is well filled with that milky substance that must form the flour. These insects deposit their eggs in the epidermis of the straw; and when these eggs are hatched the emerging insects feed on the parenchyma and destroy a part of the vessels; that part from which the head is derived; and, consequently, the grain, not receiving all its material, remains meagre and light, in a word, *shriveled.* It is recognized that the wheat is attacked by insects from the black spots on the straw; for it is believed that these spots are their excreta."

Cult. des Terres. Page 220.

ARTICLE II

Description of the principal diseases of Wheat

The different diseases of wheat presented in the foregoing are not, as one may see, those that should be the most dreaded; since, if one excepts the rare effects of a hard freeze that may destroy absolutely everything, the disease consists only in the meagerness of the heads and the diminution of farinaceous substance: real injury, in truth, but

much less than that where the heads are totally blasted and almost never filled with sound kernels. The diseases that result in the entire loss of the head are of three sorts: in considering them, let us follow the order of time in which they appear. The first, less known but as serious as the other two, is that in which one notes a considerable derangement of the stem, the leaves, the head, and in the kind of grain with which the head is filled, before the wheat plant blooms and while the stems, still tender, are not more than a foot or a foot and a half high. The stems of wheat plants of this sort, which I shall distinguish from the others by designating them as *aborted* plants, are ordinarily shorter than stems of the same age; they are twisted, gnarled, and rachitic. Their leaves are commonly of a bluish green color, curled up in different directions; sometimes turned in a double manner, sometimes showing a slight spiral sinuosity, or presenting quite distinctly the figure of a worm-screw.

Aborted Wheat Plants

Although the loss of grain may always be the result of this disease, the circumstances that accompany it are, however, not always the same; the alterations, be they of the stem or of the leaves or of the head, are unequally marked and I have seen such aborted heads whose stems were straight and whose leaves were only slightly curled. The head, in entirely aborted wheat plants and in which the disease is at its height, retains only very little of its normal aspect. It is pinched, dried, and shows only the very imperfect beginnings of as many little glumes that must envelop the grain as of the grain destined to form there. In wheat plants, where the blighting is externally less manifest, the stem is quite straight, the head is formed, the leaves are slightly twisted, the glumes, although shorter than those of healthy wheat, remain entire; but, instead of containing a little white embryo, velveted at its summit, as it is toward the time of flowering, they cover merely a greenish kernel terminated abruptly by a point, and quite similar to a little pea that begins to form in the pod. These green kernels often have two well-marked points, sometimes they have three of them and are configured in a manner seeming to indicate that there may be two or three kernels that, at first, were separate and later may have been united by starting from the same base and by growing within the same glumes.

Without doubt, it may be recalled at this moment that Mr. Wolf regards the abnormality of wheat kernels as a source of disease, and one is tempted to believe that he knew the kind of disease I am about to describe. I, myself, have been of this persuasion, especially in consulting M. Manget, who has commented on the contribution of Mr. Wolf, and who seems to desire to characterize our blighted kernels, by designating them, according to the Act of Leipsic, *monstra tricorporea*. But one will not be slow to recognize that Mr. Wolf, in speaking of abnormal kernels, did not mean those of *blighted* wheat plants, and that if I, myself, have been in error on this point, it was solely because I had not noticed a mistake that had escaped Mr. Wolf. I return now to that that concerns the first of the major diseases of wheat.

Bibliot. Scrip. Med. Tom. 4. Page 644.

When I stated at the beginning of this article that the disease of blighted wheat plants is as serious as the other two diseases, which soon will be before us for consideration, I well realized that certain farmers were not at all in accord with me; in fact, I know by experience that some of them do not know the former disease. If, in glancing at a pile of wheat, they perceive some blasted black kernels, they confuse these with the seeds of cockle, a plant that grows among the wheat plants, whose seeds are black and of nearly the same shape as the smut balls. They do not realize that these bad kernels of wheat constitute a very real, sometimes a very considerable, crop loss rather than a slight increase, as they imagine, in taking them for seeds of cockle. If they are troubled with two diseases, in which the kernels are reduced to a black dust, and especially with the one where the seeds, although ruined, retain their outer envelope, it is less so because of the resultant diminished harvest yield than because of the black dust that comes out of the diseased kernels when the bundles are thrashed and sticks tenaciously to the sound seed, contaminating especially the brush end of each kernel and leaving there a black blemish, quite aptly described by the term *smudge*. This blackening is in reality only a purely external disadvantage, if one considers the kernel as alone designed to serve as food, and one need have no fear of its exerting a bad effect on the health. But the smut-laden kernels are not at all agreeable to the eye, the bread made from them is not perfectly white: also, this

grain is subject to a dockage of one-fifth or even a fourth of the current price when it is offered for sale.

Smutted Wheat Plants.

The second of the major diseases of wheat is of such nature as to command notice the moment the ruined head is free from the sheath; it is not nearly so easy to recognize while the head is still hidden within the sheath. It consists in the almost entire destruction of the head, of which there remains only a kind of core, along the length of which the kernels were attached, which now consist of nothing more than black, dry dust, and some white threads, sole remnants of the envelopes that surrounded the kernels; these remains are commonly the awns that are found at the tips of the glumes and that are hardy enough to resist disease. In these heads thus destroyed, and that I shall call *smutted* in order to give one an appreciable idea; in these heads, still within the sheath, one finds some kernels covered with a white epidermis, very thin, transparent, and only meagerly developed. These kernels are black within; the scant envelope that covers them and adheres within the sheath, probably dries on being exposed to the air; the entire head, emerging at once, hardly differs from those heads that we have said consisted only of a dust and of awns, remanants of the tips of the glumes.

Bunted wheat.

The third disease of wheat, which constitutes the principal thesis of this memoir, is that in which the kernel, retaining almost its normal form and its own bulk, is converted into a greasy and blackish powder, and becomes easily detached from the base of the glume when it is very dry: I shall designate it in due course under the name of *bunted* wheat. One begins to distinguish, before the completion of the flowering period, the most advanced heads among those attacked by this disease. As long as the heads are enclosed within their sheaths, even in full daylight, one suspects nothing wrong with the plant; the stem is straight and upright, the leaves are commonly without defect; but when the wheat plants blossom, the bunted heads are recognized by their bluish color; the glumes, which envelop the kernel, are more or less spotted by little white patches; the kernel itself, larger than it ought to be naturally, is very dark green; so long as it retains this color, it adheres to the base of the glume like a sound kernel; its three stamens, much less extended than it [the bunted kernel] and adherent

to its sides, drooping and withered: if one opens this bunted kernel, one finds it filled with a greasy, blackish material, from which a fetid odor is given off, especially when one crushes the bunted kernel between the fingers.

Under the name of *smutted wheat* M. Duhamel apparently includes the second and third disease;[2] although both seem to have the same origin, I have felt that we should separate them. They offer, in fact, some quite noticeable differences. The second develops more suddenly than the third; the glumes of the *bunt ball*, even to its epidermis, are very little altered, while in the *smutted* plants they are entirely destroyed. The dust particles of the *bunt balls*, viewed under the microscope, are larger than those of the *smutted* kernels: In a word, nothing remains of the heads of plants affected with the latter disease, neither well-formed kernels nor pistils nor vestiges of stamens. That is to say, everything is consumed and reduced to a dry dust, which the least agi-

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[2] This memoir was not yet before me when I received M. Duhamel's contribution entitled *Series of Experiments, etc.* 1752. In the 3rd chapter of this paper, the author speaks of *black wheat, blighted or smutted;* he made the distinction between the two diseases by the black dust into which the substance of the kernel is changed.

By these words *Nielle proprement dite*, he means the disease that I have designated by the name *smutted wheat;* and by the *malformation or smut*, that which I have indicated under the name of *carious or bunted wheat.* I was surprised to find in this publication no observation on the peculiar condition I have included in the number of the principal diseases of wheat under the name of *aborted wheat.* Perhaps it is only rarely observed in the cantons that have come under M. Duhamel's observation; or perhaps the aborted kernels have been confused with the carious ones in assuming, like some growers of this country, that there are years when the kernels of diseased wheat crush under the fingers and become reduced to a black dust, and other years when they simply dry out and yet retain a certain consistency.

In the description I have given for the blight disease it is thought well to distinguish it from the other two diseases of which M. Duhamel speaks. Moreover, I am able to state that it is very common in the different cantons I have inspected. I carefully retained several of last-year's wheat plants that were attacked by this kind of disease and that have been subject to my local observations, without counting those I have made at large on a large number of other plants affected by the same disease.

The work of M. Duhamel, now under consideration, has not failed to interest the public. Always due consideration is given to new principles of soil management. It is learned with pleasure that in different countries enlightened men, zealous citizens, confirm by proper tests the soundness of those principles and recall the best days of Rome in vouchsafing their devotion to agriculture without any loss of personal worth.

Aside from the interest I take, as a citizen, in this entire work, I, as author, call special attention to its third chapter. It gives me opportunity to do justice to M. Duhamel whose elucidations have been very helpful to me. I have noted there that the observations of this celebrated Academician square very well with mine without causing one to suspect that his last contribution, which did not reach me until after I had sent him this memoir, had served to guide me and put me on the way. I note in this same chapter that M. Duhamel considers, as I do, that nearly all the causes to which the principal diseases of wheat have thus far been attributed have been advanced without fundamental basis; and I have noticed there, unless I am mistaken, several things that, well developed, agree with the opinion that repeated experiments will lead me finally to adopt.

tation precipitates. In the bunt balls one perceives no noticeable derangement; the glumes adhere; the kernels, although inwardly reduced to dust, retain almost their normal shape: one still sees the velvet-down of their tips, the pistils and stamens remain: the disease in the *bunted* heads is almost wholly internal. In the *smutted* heads, it seems as though there had been a fire; the loss is complete.

It is recognized that we must distinguish the two different diseases of wheat, and, in naming them, we must not employ the too general names of blast, blemish, frost, etc. It is still more necessary to realize that the blighted kernels, in particular, do not contain any dust like that of *bunt balls* and like the *smutted kernels*. The blighted kernels are as green as the culms that bear them and are filled with a glairy substance; they dry at the same time the stems do, shrink, and become black, but retain their shape, nevertheless, although withered and void of sap.

It is not necessary, however, to regard these three separate diseases of wheat as so distinctly unlike that they may never be confused, although the symptoms of the one are never those of the other, and although they may always be designated by all the indices that we have said accompany them. Among the great number of diseased wheat plants on which I have made observations, I have sometimes found some *bunt balls* in certain heads affected also with *smut;* some blighted kernels in some heads of which the majority of the kernels were *bunted;* some straight stems that bore blighted kernels; some twisted leaves on *bunted* wheat plants: I will say even more; I have encountered some heads laden with sound kernels, although borne by tillers whose leaves were curled, and indicative of blight. But these cases are all rare; the differences presented by the principal diseases of wheat generally are such as we have established: if there are some exceptions to be noted, one should disregard them and consider as constant in this article only that that most commonly occurs. I shall add here an observation that we have not had too clearly in mind, because it relates obscurely to the three kinds of diseased kernels I have differentiated. That is to say, these spoiled kernels are sometimes found associated in the same head with one or several normal kernels; or develop on tillers

belonging to a root system from which other tillers may spring, laden with sound grain.

It is opportune now to examine the opinion of Mr. Wolf on the cause of the disease of grain: I realize in advance that this discussion will tire the reader; but it is desirable to give attention to the opinion of a distinguished scholar whose high reputation has already had some influence on this subject.[3] One owes to great men, when one dares to take issue with them, the right to develop their ideas and to make easy to the public the means of presenting them.

Mr. Wolf, without distinguishing between smutted and bunted wheat plants, contents himself with saying, "that it often happens that the kernels of a head of wheat become black as coals, and soften finally more and more until at last they dissolve into a black dust, fine as flour." A con- § 1
dition to which he gives a general name, such as that of *smut*, in our language. After these few words that relate to wheat, the subject, as presented in the chapter by Mr. Wolf, is one of no further research interest to him. He cites some experiments, but they are made on barley and oats: his special tests are applicable to wheat, only, in so far as what he relates concerning the seed of barley and oats may, according to him, apply to wheat, and also, as he believes, the diseases of these three crop plants have one and the same origin. Let us enter now into the matter of finding out what Mr. Wolf observed in barley.

"When in 1716," says he, "I tried to determine by ex- § 14
periment the cause that I had assigned to the multiplication of wheat plants, chance favored me to the extent that a single kernel of barley, planted in a box that I kept in my bedroom during the night and exposed during the day at an open window, without rain or artificial watering, produced some healthy heads and some that were smutty. . . . When the smutty heads formed I observed at first a considerable difference in the beards; for, instead of the beards of a head of barley being long, straight, and green, like those of a healthy head, those of the smutty head were much shorter, curled, twisted, and whitish. The following I perceived early in the smutted heads.

[3] In the *Oeconomic Journal* of September, 1751, page 13, and following, there is a memoir on smut, in which the opinion of Mr. Wolf is accepted. The author of this memoir regards as constant the abnormality of kernels of wheat reduced to black dust.

§ 17 The smutted grains were more exposed than the sound ones, for the most part black, and only here and there spotted with green. Their shape differed noticeably from that of the good kernels; for, toward the distal end, the kernel was divided into three parts, as if there had been two other kernels ready to grow; or as if three kernels were joined together in a manner to form one; with the help of a magnifying glass, moreover, one could see clearly that a
§ 19 grain of smutted barley was a monstrosity made up of three bodies. As all the kernels of my smutted head were of the same form, I walked through the fields where I found several heads of smutted barley the grains of which were all similar
§ 20 to those that I had at home. . . . Thereupon, I recognized clearly that the smutted kernels were nothing other than monstrosities: and, since liquid materials spoil when they
§ 21 stagnate, I judged that the sap, which circulates in all plants, had stopped in its movement, because of the abnormal shape of the kernels, and spoiled because of stagnation. . . . How does it happen that nature produces abnormalities among wheat plants? This I make no pretense of determining here. . . . However, it is seen that up to the present there is no remedy against smut, and, that, as a consequence, it will be necessary to continue to perform experiments in order to discover the causes that produce an abnormality in nature."

In order to present more clearly the opinion of Mr. Wolf and be able to speak from my own observations, I examined carefully several smutted heads of barley held over from last year. These were heads of common barley, generally designated *Hordeum distichum*, and not of another species known under the name of *Hordeum polystichum*, the heads of which are of an unusual shape.

The diseased barley grains that I have observed have quite the form ascribed to them by Mr. Wolf, that is to say, three grains united to form one. The envelope that covers these smutty grains is nothing other than the glume itself; in fact, it is known that the envelope proper of the barley grain is extremely delicate, and that the glumes, which one can not too much admire, adhere to the grain itself, thus making up for the delicacy of the true envelope. This sort of artificial envelope of the barley grain, comprising the glumes, is the only one left on the smutty grains. It is not,

however, so well preserved as that of the bunted wheat kernel. One rarely finds grains of diseased barley that are not ruptured at some point and that thus expose to view the black dust with which they are filled.

In carefully examining the heads of barley above referred to and comparing them with heads of healthy barley, also held over from last year, I have pointed out that in the sound heads the well-ripened grain is joined at the base by two false grains, which always abort, and at the expense of which the middle grain is nourished. From that moment I have been of the opinion that the malformation caused by the smut might bring about the almost complete fusion of the three grains. Mr. Wolf observed that all smutty grains of barley have the same configuration, that they present three apices, and are like three grains joined to form a single one. It would be very unusual if a malformation, an imperfection in nature, varied in no respect in different plants, in barley of every country, if this deformity had not everywhere the same cause. I wish to speak here of the existence of three bodies that are easily distinguished in their normal state but are found united through some accident and are unable to develop as individual grains.

I come now to the very foundation of Mr. Wolf's theory. He considers the black dust of the diseased grains to be the result of a defect in the circulation of the sap within the malformed body. This opinion seems specious if in the light of it we care to consider smutted barley grains as so many malformations. It would appear quite plausible in relation to aborted wheat grains, which may be considered even more malformed than smutted grains of barley. But will this opinion hold when applied to the bunted grains of wheat? These latter are not to be regarded as monstrosities. On drying, they maintain very nearly their natural shape. Internally, however, they are reduced to black dust without any outward indication that the movement of the sap had been checked there; without one's justifiably suspecting that the sap, in thus stagnating in the bunted kernels had there become decadent material, for the cessation of the movement of the sap is attributed solely to the deformity of the grain.

Let us conclude with the detail here recorded.

(1). That Mr. Wolf, in speaking of malformed grains of

wheat, did not mean those to which I have applied the term *aborted*. The 3-body abnormalities of which he speaks are grains reduced to black dust; they belong to the order of smut grains, whilst the grains that I have described and have observed only in wheat contain no dust and must be separately classified.

(2). That Mr. Wolf meant by malformed grains of wheat neither bunted grains, since their shape is well maintained, nor smutted grains, since it is very seldom one finds them intact in the head, except in the early stage of the disease, a stage very difficult to obtain, but one in which the grains possess their characteristic form and differ from the sound kernels only in some black spots with which they are blemished.

(3). Finally, that the barley disease is the only one Mr. Wolf has carefully considered. That he was struck by the abnormal shape of the diseased grains of this plant; that he believed he saw in their malformation an evidence of disease; that, without examining the shape of the wheat grains that had been converted into dust and those of oats attacked by the same disease, he applied to these two plants the same theory he had applied to barley.

ARTICLE III

On the common disease of Rye known as ERGOT OR SPURRED WHEAT.

Before closing this chapter I believe I should recall here that singular disease of rye known under the name of *spurred wheat* or *ergot*, because, in fact, the grain of rye thus attacked has somewhat the appearance of a cock's spur. This disease bears some relation to my subject: I have noticed it with surprise on wheat, and without doubt it is due to one and the same cause in the several plants we shall find subject to it.

Languis, physician and learned naturalist, is one of the contributors who has the more carefully examined the disease of rye and who has gone into the greatest detail either in describing the deplorable effects, which, according to him, follow ergot, when he finds it mixed abundantly with bread, or in depicting with exactness this defective grain and in explaining the cause to which he believes it is to be attributed. In fear that I may wander too far from

my objective, I shall not cite the observations made by him as a physician. One cannot read them, however, without becoming keenly interested in them from the standpoint of humanity. I shall, therefore, content myself with relating summarily the place where he recorded the description of ergot and where he tried to explain the cause of it[4] *Clavi Etc.*

I do not know whether the distinction made by Languis between one kind of ergot, very baleful in itself, and another that is not at all so, is based on experiment. I would willingly believe that this kind of diseased seed always is of bad quality, but it is noticeably injurious only when a certain quantity is mixed with the rye flour. This idea is supported by consideration of such years as 1709, when the sad effects of ergot were especially pronounced. It was then more abundant than usual, since, according to the report of M. Noël, Surgeon of the Hospital of Orleans, who transmitted some observations on the subject to the Academy of Sciences, nearly one-fourth of the rye of the Sologne consisted of ergot grains, which the poor people neglected to separate from the good grain because of the extreme scarcity following the long winter. We shall now return to Languis who explains the cause of ergot.[5] *Excedenti vero aeris* Etc.

Hist. de l'Acad. des Sciences 1710. Page 63.

I shall not stop to raise objections to the opinions of Languis, who attributes ergot to excessive atmospheric

Description merborum ex e su clavorum secalinorum cum pane. Lucernae 1717. Act. Lips. 1718. Page 309.

[4] *Clavi (l'Ergot) Sunt morbus granorum secalinorum qui in eorum excrescentiâ. magnitudine auctâ & formâ coloreque depravato consistit. Sunt hi clavi, accuminatâ suâ parte vaginæ implantati, instar reliquorum granorum, quam, propter crassitiem suam, nimis distendunt; coloris fusci, nigricantis, duri, modò teneriores, modò crassiores, recti, vel incurvati, aut cornuti, saporis frumentacei, cum acrimontâ tamen conjuncti; juxtà longitudinem binas, ternas, vel plures nanciscuntur fissuras . . . Dum conteruntur non farinam volatilem concedunt, sed pulverem graviorem . . . Accensi clavi facilè flammam concipiunt, sed obscuram & nigriusculam & cinerem post se relinquunt nigricantem, splendentem tamen instar cornuum vel crinium combustorum. Non omnes clavi infaustos edunt effectus, verum venenati tantum; quotannis enim tales in spicis frumenti concipiuntur; omni tamen venenositate carentes Venenosi in egenis polissimùm deleteriam suam exerunt vim ut potè qui nonnisi secalino vescentes pane nullam in clavis à bonis granis separandis aut frumento debitè exsiccando adhibent curam.*

[5] *Exedenti verò aeris humiditati adscribendus est nimius clavorum proventus, cui ros, nebula, pluvia socias adjugunt manus. Aer enim humidus, ratione subtilium & levium suarum particularum, fortiter penetrat, nitro, sulphure volatili aliisque subtilibus particulis imprægnatus. Hinc spicam ambit vaginam emollit, extendit, premit involucrum farinaceum ad putredinem disponit, cum pericarpio se uniens in fermentationis motum rapitur, sicque augmentum grani promovet . . . Venenositas (granis) accedit à rubigine, ut potè quæ ao potissimum tempore descendere observatur quo grana frumenti in clavos excrescere incipiunt; sub formâ etiam materiæ cujusdam viscidæ dulcis eis adhærere inventa fuit, ac tandem iisdem gaudet facultatibus, quibus clavos nocere diximus, visciditate putà & acrimoniâ . . . Rubigo ex aequæis, insalubribus tamen ac venenatis constat particulis quæ de die ex terra & aquis impuris sublatæ vel per ventos adductæ, noctuque condensatæ manè delabuntur . . . Gaudet hæc rubigo visciditate dulci cum corrosivâ tamen acrimoniâ conjunctâ.*

humidity, dews, mists, and rain; who claims that very moist air laden with nitrous, sulphurous, and other volatile components, impinging upon the epidermis of the grain, softens it, renders it liable to contamination, even to marked penetration of the grain, excites within it a fermentation, swells and extends it excessively; and who claims that the harmful quality of certain ergotized wheat depends on a viscous, acrid, corrosive substance, very noticeable in plants and occurring especially at a time when ergot begins to form. The reflections that I make at this moment on the opinion of the celebrated physician, who attributes ergot to the malignity of certain mists will apply naturally to those of Languis.

Hist. de l'Acad. des Sciences 1710. Page 63.

The Historian of the Royal Academy of Sciences says, in considering the explanation given by M. Fagon for the origin of ergot (b), that "there are mists that injure wheat and against which the majority of rye heads are protected by their awns. In those that this malign humidity can reach and penetrate, it disintegrates the pericarp, which covers the grain, and blackens and alters the substance of the grain itself. The sap, no longer confined within ordinary limits by the pericarp, is carried into the grain in greater abundance and, collecting irregularly, forms a sort of malformation, which, moreover, is harmful because it is composed of a mixture of this superfluous sap with impure moisture. It is only in rye that one finds ergot. . . . This ill-appointed kind of grain, it is stated farther on, occurs, according to a naturalist who had consulted M. Delahire, in great abundance on damp, cold soil and in rainy years. A certain variety of rye, which is sown in March, is more subject than those varieties that are autumn-sown.

I feel that M. Fagon has some weight of authority, but I can hardly understand how the mists, said to be the cause of ergot, fail to produce this singular malady in barley, oats, etc. If one considers the awns of the head as being able to prevent the mists from penetrating the grain itself, this explanation no longer stands in face of a prodigious quantity of heads of wheat that are awnless and in which one almost never finds ergot. Moreover, one might call attention to the fact that the mists ordinarily cover a certain extent of ground and, being supposedly injurious, ought to produce a quite general effect. Experience does not here square with

theory; a given head is ergotized, its neighbor is not; you run through several rows where the heads are healthy, suddenly you see two or three with ergot; sometimes an *arpent* shows ergotized wheat at every step, while that of another lot alongside shows none whatever, or but very little. An affected head of rye, on the other hand, bears one, two, three, up to eight ergots, according to M. Dodart: these cornute grains are more or less bulky, more or less elongate. I have kept several of them intact in the head, one among others of which the shape approximates that of a curved spindle; it is one-and-a-half inches long, over and above its curvature, and about two lines in diameter at its thickest point; but I have never found a head of rye in which all of the kernels were ergotized; and I have never read a memoir on this subject in which mention of an entirely ergotized head was made.

Mém. de l'Acad. des Sciences. Tom. 10, p. 562.

Without doubt, it would be vain for me to point out the fact that one is in error in attributing to humidity the genesis of ergot, as this disease attacks rye sown in elevated situations as well as that in the lowest situations, and during dry years as well as rainy ones. I shall merely note that M. Fagon deceives himself in asserting that ergot is a malady peculiar to rye: I have observed this ergot and have frequently seen it on a little plant known among botanists under the name of *Gramen loliaceum*, on *Gramen micosuros*, and on true darnel grass. Here are the facts on the subject of these three plants that should be brought out; facts that banish every notion attributing ergot to humidity.

First Fact. One of my friends[6] has shown me some darnel grass that gave no indication of ergot, although it came from a place that had been inundated by spring water, and where this unfortunate condition had brought about the complete failure of rye that had been sown there. This friend, exact observer that he was, assured me that there was in this same inundated area a considerable quantity of darnel grass; that it was very vigorous, judged from the

[6] This friend, whom to-day I am free to name, is M. Ludot, known among men of letters through a prize obtained by him in the Royal Academy of Sciences in recognition of rare talents and a modesty even yet more rare than his talents. I am indebted to him for numerous nice observations on the subject under consideration. He has communicated them to me with that disinterestedness, and I dare say, that simplicity that characterize a gracious spirit and that nearly always mark the superiority of the enlightened.

number of culms as well as from the length of heads and abundance of seed; that he had examined it carefully but had, however, perceived no ergot.

Second Fact. In a place, also inundated by chance springs, where some wheat had been drowned beyond recovery, there were found four or five tufts of *Gramen micosuros*, comprising 70 to 80 culms each. Ergot appeared in only one of these tufts; yet there were but 12 or 15 culms of this one tuft the heads of which bore one or two ergotized grains.

Third Fact. Ergotized *Gramen loliaceum* is found from August to October, inclusive; it develops everywhere, indifferently during dry as well as wet seasons; it grows in the middle of a field, on a high place, on the borders of a little ditch, in a trench; I have noticed it in abundance on the bulwarks where it was certainly well assured against moisture.

In declining to accept the explanation M. Fagon has given for the development of ergot, I hesitate only to consider mists as the primary cause of injury and I accept without reservation what he says about the formation of ergot itself.

In fact, I believe that in assuming the cause to be due to the sting of some insect, rather than injurious humidity, one may state with some probability and ovbiate all difficulties that the pericarp of the seed becomes half-opened following this puncture; that the sap there present, being no longer confined within ordinary limits by the pericarp is present in great abundance and, accumulating irregularly, produces a sort of malformation. Let us add also that this insect may leave in the grain it has punctured a subtle and vinous liquid that ferments in the farinaceous juice tainted by it, and may serve to explain why ergot copiously mixed in bread becomes a kind of poison and produces in those who have eaten it scorbutic troubles, and sometimes occasions gangrene: while the diseases of wheat, barley, and oats never produce any subsequent bad results.

Let us recall here a point in the description of ergot given by Languis: *Juxta longitudinem (clavorum) binas, ternas, vel plures nansiscuntur fissuras.* The fissures that this author has noticed along ergot grains may, without doubt, result from desiccation subsequent to excessive swell-

ing. But would it be extraordinary if these openings had their origin in the pericarp puncture produced by an insect? Would it be surprising if they took the form of longitudinal fissures, since their edges are pushed apart by means of an excess of farinaceous material there present and, since these cracks in time are evident after a part of this material has become dry and shrunken and the ergotized full-grown grain has, by this time, become wrinkled, black, and completely withered?

In attributing ergot to the sting of an insect I have ignored no opinions received. M. Marchant has given a description of a kind of tuberosity or extraordinary growth he observed on a radish and that he believed was caused by insect puncture. One should consider also as the result of a sting those swellings or galls that one sometimes observes on *Sinapi arvensa minus*, whether on the stem or on the branches or in the region of the flowers. The excellent memoir of M. de Reaumur on the galls of trees, shrubs, and herbaceous plants; the surprising details into which he has entered relative to the different insects whose stings engender galls, should familiarize us with these singular but purely natural results and show us their similarity to what I, perhaps the first, have advanced on the cause of cornute wheat. It will undoubtedly appear to have been proper to have reported at the beginning of this digression the substance of a memoir of M. Dodart on cornute wheat, one of the first contributions made to our knowledge of this sort of grain disease. But, in speaking of ergot, I have had in view principally the authors who have, themselves, tried to explain the cause of it. The description of cornute wheat given by M. Dodart is much briefer than that by Languis. If he attributes ergot to excessive heat followed by moisture, it is only after several letters written to him, which vary, moreover, among themselves, that an attempt is made to detail the evil effects of ergot. It is of this contribution that M. Dodart has made the abstract. I have observed there a statement that supports my view, contrary to the distinction made by Languis between injurious cornute wheat and other wheat not thus affected. There is scarcely a year, he says in the abstract, when a few of these grains do not develop; when there are but few, one does not notice the bad effect.

Mém. de l'Acad. des Sciences. 1709, p. 64.

Hist. des Ins. Tom. 3. Mém. 12.

Mém. de l'Acad. des Sciences. 10. page 562.

La Mérid. de Paris, suite des Mém. de l'Acad. des Sciences. 1740. 3me partie page cxiv.

I shall close these elucidations on cornute wheat by citing a passage from the natural-history observations made in one of the provinces of central France, by M. Lemonnier. The authority of this scholarly academician fortunately threw me into perplexity on a point that I had always regarded as certain. Following is the passage, faithfully excerpted: "The crops (in the southern part of Berri) are often damaged by hot, humid winds, which prevail during the months of June and July, and one sees then much blasted or ergotized wheat."

Wheat, therefore, also is subject to ergot. I at first suspected, on reading this observation by M. Lemonnier, that he would be able to avoid giving to the kind of disease he had observed in wheat the name ascribed to the disease of rye. I know that all those who have written on the untoward circumstances to which wheat is exposed had never included ergot, and I was then able to assert that the great number of heads of wheat that had passed through my hands none had contained any ergots. But I am now certain that a grain of wheat can become ergotized. The case is, however, extremely rare; I have yet noted it but twice in different parcels of wheat situated in the neighborhood of Troyes.

CHAPTER IV

Special observations on bunted grains of wheat. Conjectures on the cause of this trouble

I HAVE intimated in the foregoing that the sting of some insect might be the cause of the rye disease. I made the statement on the bold presumption that I might thereby solve several difficulties, and find support in examples that very carefully examined plants quite frequently furnish. If, without daring to assert that insects do injure wheat, I suggest farther on some convincing opinions, it will no longer be on a mere presumption solely with a view to accounting for everything and satisfying myself with some recognized effects. I shall cite some exact observations; I shall indicate the little creatures that one might suspect of being the originators of the trouble; and I shall enter into details that, even though they may have no direct bearing on our objective, do bear at least some relation to it and perhaps may sometime be found pertinent. It is well known that I have kept an accurate journal of every interesting thing I have found in my examination of wheat. But this record contains some data that have not been at all sufficiently verified and others that have been noted several times: I shall therefore be content to give the substance, hazarding nothing, and present simply what I have observed.

Convinced as I was last spring that the disease of wheat should appear early and attack the still tender stems, I went through several plots of wheat in which the culms had begun to form. Up to the time when they were only seven to eight inches high I noticed no derangement; but when they were about a foot high, the first thing to strike me was the inverse rolling of the leaves; I examined them carefully and noted the fact that they frequently belonged to twisted as well as knotted stems. At time of emergence of the heads, I was particularly concerned with observing these malformed stems; I carefully examined their heads and at once removed the glumes in order to examine the enclosed kernels.

Instead of finding a white embryo, velvety above, such as a sound kernel shows in its incipiency, I generally found only some small green grains terminated by one, two, or three points, often exceeding in number what a single head should produce, sometimes joined together in a sort of malformation, such as we have already spoken of.

I at first believed that this disease of wheat was the one in which I was particularly interested and that these little green grains, once having attained a certain size, would become converted to dust and be nothing other than carious kernels. With this idea, which I held for some time, because the moment for recognition of wheat affected by this last kind of disease had not yet arrived, I marked several malformed stems so as to distinguish them on occasion and to follow in them the progress of the disease. But, the more I advanced in my observations, the less did the little green grains appear to change into bunt grains. Instead of swelling and assuming shape, they became a bit desiccated, their envelope turned brown, and their glistening substance became reduced to nothing.

In early June, at a moment when I was without anything distinct to observe, I forced open the sheath of a head, more backward than the others and borne on a malformed stem. I saw a swarm of tiny black insects come out, creatures alone rendered noticeable by their color. They spread out over the culm, crawled about for a moment, and then reentered the sheath they had left and that had automatically closed. I devoted myself from that moment to a consideration of all culms thus harshly dealt with; I examined them carefully, and I commonly found there insects similar to those first observed, I collected and carried to my home a little sheaf of these malformed stems, bearing inversely curved leaves, in the sheaths of which numerous insects remained enveloped.

These little creatures belonged to the *Staphilinidae*, insects that, like the earwigs, have very short wing covers under which their wings, large enough to support them in the air, and like transparent gauze, are admirably folded. They have two antennae formed of several segments placed end to end like the beads of a chaplet, the last segment terminating in a point; some hairs are noticeable at the point of juncture of the first segments to the side of the insect's

head. These basal segments are of a darker color than the others. I stated that the antennae are composed of several segments because their number is not fixed for each insect; that depends, according to every appearance, on the age of the insect. The antennae of the youngest consist of but two or three segments. The older ones commonly have up to six. I have seen some insects in which one antenna was longer by a segment than the other, due, no doubt, to some accident. These insects have six legs of unequal size, each composed of two articulations; their bodies, from the extremity of the corselet to the tail, appear to me to comprise nine rings; the tail ends in a point and bears at the tip two hairs disposed in the form of horns.

Assuming that these insects might cause considerable damage to wheat, being commonly found on wheat in the different cantons in great numbers and indistinguishably, I examined these little animals carefully; I continued to open the sheaths of the heads in which I suspected some damage; I pushed aside the glumes which enveloped the grains; I often saw there some insects, but I did not make out their behavior nor what part of the wheat plant they had altered: I clearly perceived that they were ordinarily more numerous and most abundant on the malformed, bluish culms bearing inversely rolled leaves; but I never chanced upon the initial moment of their attack; the injury, if they really produced it, was done at other times than those of my observations.

About the time when I first discovered the insects, that is to say, before the wheat blossomed, I noticed, and this observation was the same in the different cantons, I noticed, I say, several heads entirely free from the boot, ruined and reduced to a black dust; I found others similarly ruined, althought well enclosed within their sheaths and showing no marked indications of deterioration. These heads, to which we have given the name *smutted* heads, only rarely showed any insects; it seemed to me that it [the smut] was more often found on enclosed than on exposed heads.

When the plants finally blossomed, several heads were observed in which the glumes and grains were dark green. I opened these grains; they were filled with a greasy material, blackish and foetid. I recognized these heads to be especially those I was seeking; in examining them carefully, I acquired the facility to recognize them at a glance; I col-

lected a certain number of them and took them to my home where I could examine them at leisure. One of the first things about them that impressed me was the discovery of some red insects mixed with the black ones, but a little smaller than the latter: I observed them under the microscope; they were wingless; but, with this exception, their configuration and their parts were the same as those of the black ones. I did not doubt but that the red and black insects were of the same species and I confirmed this conclusion by the lack of uniformity of color in the new insects, some being a brighter red than others, and yet others that had little black spots on the back indicative of a metamorphosis and a change from red to black.

The red insects being more common than the black during the flowering period of the wheat, I had every possible facility for their observation, and I found them quite often even on the sound heads. The examination that I made of several glumes covering the bunt grains was very helpful and threw some light on the subject of the insects; it convinced me that these little creatures made their home ordinarily in the heads of wheat and that they laid their eggs there; that from these eggs came worms; and that these worms, after passing the condition of nymphs, became in time the insects I observed. I noticed several times, in fact, and in a manner beyond question, their excreta on the glumes of the bunt grains; I noticed there some yellow shells where the insects were in the nymph stage, while others were white and open at the end whence the insects had emerged. I frequently found on the glumes or on some of the grains little dead insects still clinging to their cast-off remains and others that were scarcely born and with antennae composed of but two segments. It was not on the bunt grains alone that I noticed insects of every color and age, their excreta, their cast-off remains; I found them also, though less frequently, on the sound grains and within their glumes; I sometimes noted, by the excreta remaining on the velvety hairs of the grain, that some of the insects had crept over a sound grain.

These observations indicated something, to be sure; but they taught me nothing concerning the damage the insects might work within the head and the kind of nourishment they derived there. In the hope of being better informed, I

decided one day to put under a microscope a sound incipient kernel the flower of which had but recently passed. The two pistils and the top of this kernel were still laden with dust from the stamens. I placed on this grain a red insect and observed it attentively. This insect made at first several rounds over the surface of the grain, finally it slipped between the hairs of the tuft at the top, crept amongst them a long time, and, in spite of the difficulty experienced because of the length of its antennae, suspended itself by one leg to a filament of one of the pistils, returned to the grain itself, reentered the tuft, plunged its head among the hairs by bringing its antennae together and resting motionless in this situation, without making other movement than that of the tail, which it moved only at intervals. After having left it at rest nearly a quarter of an hour, I agitated slightly the grain on which it lay; it did not move, at all, and I had to touch it with the point of a pen in order to make it take leave.

I repeated the experiment several times by placing insects on the grains; they crept willingly, mingled straightway with the hairs of the tufted top, plunged their heads among them, leaving them momentarily, then returning, as if to a place they had found agreeable, and rubbing their legs against each other in order to remove the dust of the stamens that had become attached there.

After several observations made on insects that I had by chance found on some of the grains or had expressly placed there, I have not perceived that they penetrate some spot and obtain nourishment from either the grain itself or from the pollen scattered over the tufted top. It was observed, however, that a red insect had thrust its head between the hairs at the distal end of the grain and that it remained there firmly attached. Might one not presume that it sucked therefrom some liquid with which the base of the hairs was moistened? Does not this liquid come from the particles of the fecundating dust, which, dilated by heat, swell and open and thus permit their oily juice to run along the hairs, thereby serving to moisten the tip of the grain? I scraped onto a small concave glass a certain quantity of these fecundating dust particles. They yielded several droplets of a very clear liquid. I exposed to the sun some of these very dust particles under a small receptacle; the top of

the receptacle became moist as though it had been exposed to the vapor of hot water. This idea of an oily sap coming from the staminate dust resulted from observations made by an able master whom I shall cite in quoting M. de Réaumur. "M. Bernard de Jussieu made a microscopic study of the staminate dust of many kinds of cruciferous flowers, such as those of mustard, rocket, etc. He studied, I say, these dusts while they were in the water in which he had placed them. He observed that these little grains swelled more and more until they cracked. At the moment when each grain cracked, there came out of it a jet of liquid, which floated on the water without mixing with it and, which, therefore, must have been an oily liquid. The same experiment was repeated, with the same success, on the dusts of plants of several different classes."

Hist. des Ins. T. 5. p. 414.

Although one cannot assert too postively that the little tuft of hairs that crowns a grain of wheat is moistened by a juice produced by the staminate dust and that our insects attach themselves there in order that they may be able to procure a more substantial diet, the manner, however, in which they thrust their heads between the pistils and into the hairy tuft of the young grain would lead one to believe that they find there something foreign to the hairs of the grain and very attractive to them.

This concept that the red insects may, possibly, consume the juice produced by the staminate dust and that this diet may meet their needs, led me to believe that the black insects, being of the same species as the red, had the same taste as they, and that, being more vigorous, were able to feed on the nondissolved pollen grains, while the anthers containing them were yet tender and easy to puncture. I hesitated to accept this idea when I considered how the anthers belonging to carious grains are partly open, withered, and of an orange color, although still situated at but half or two-thirds the height of the kernel when, as a matter of fact, they should, normally, open and show indications of maturity only when they extend beyond the kernel and are about to project beyond the glumes. I thenceforth regarded the black insects as the indirect cause of the bunt grains, since these grains, not being themselves actually damaged, and, actually bearing their short stamens, appeared to me to be deranged through failure of fecundation,

a failure to be attributed, I believed, to the opening made in the stamens and to the loss of pollen. I was supported in this by an observation recorded in my journal. To be sure, it is recorded there but once; but without regarding it as confirmed, I made note of it as follows: Page 226.

"The carious grains are invariably green: I noticed that those in which some portion is white had one or two stamens taller than those commonly lateral to the carious grains and were able to manifest a little fecundation, their pistils still showing some pollen grains produced by the stamens projecting above them" [the pistils].

I did not ignore the fact that M. Duhamel, in his treatise on the culture of soils, said that he had "quite sufficiently proved that all the grains that are not fecundated do not become smutted." And I would not have hesitated to accept the authority of this scholarly academician had he spoken from some personally conducted experiment; but he explained in a general way and I did not see that he had looked particularly into what became of the nonfecundated wheat grains. As for me, I certainly had not seen any bunt grain in which the rôle of the stamens had been carried out. I observed no healthy kernel in which the stamens had been functionless.

The statements of M. Duhamel seem to me to require further explanation. The grains of wheat, considerably altered from the start, develop on stunted culms and become desiccated as they grow; these grains, I say, will not become bunted, it is true, or smutted, according to M. Duhamel's statement, even though they have not been fecundated; they will terminate in blight. But some of the wheat grains that have no noticeable defects, that develop on apparently healthy culms, and that grow from the moment of fecundation by receiving an abundance of nourishment; these grains will become diseased, bunted, if, at the moment fixed by nature, they are not penetrated by a juice capable of engendering fermentation within them and thereby developing their contained germ.[1]

[1] Grape seeds, which have not been fecundated are not ruined, it is true, and even reach maturity, remaining much smaller than the fecundated grains: but one should not always draw conclusions about one plant from another; the difference in organization is a source of varieties. One often notices that a given cause does not produce a like effect in different plants. Moreover, the grape fruit should not be compared with a wheat seed; it is the seed, itself, of the grape that should enter into the comparison

Although I realize how difficult it is to determine the actual cause, the primary source of the blight of wheat, I cannot wholly abandon the surmises that the aborted stamens of the bunted grains and the resultant failure of fecundation have raised in my mind. Perhaps some grains of wheat would not become bunted through this failure alone; possibly there is a hidden cause of both the decay and the blighting of the stamens themselves, apart from this latter being the origin of the first. But it is worth noting that there is always failure of fecundation in the bunted grains; at least, one may perhaps believe that the blighting of the stamens, the one conspicuous abnormality of these diseased grains, may be the origin of some imperfection.

One may here clearly see how I infer as solved a question that the authority of a celebrated botanist, it would appear, should render undecided. According to M. Tournefort's theory the pollen grains are merely the superfluous remains of the grain, and the filaments those of excretory canals by which the nascent embryo discharges useless juices by depositing them in the anthers. But the opinion of this eminent man is no longer held. M. Geoffroi, in an excellent memoir on the structure and use of the principal parts of flowers, has presented some new views; he has given to the stamens more worthy functions than M. Tournefort had conceived. He regards them as necessary to the fecundation of plants; he shows that the pollen of the anthers is designed to fall on the pistils, and that nature, being uniform in this particular for different plants, relates them to the common order and ascribes to them a place in the scheme of generation.

Mém. de l'Acad. des Sciences, 1711. Page 207.

To return to our objective, one really cannot knowingly consider a head of wheat, about to bloom, without being convinced that the stamens are essential parts and have been so disposed as to insure their function. The anthers borne on the extremely delicate filaments are arranged around the grain, they extend above it as they mature: When they have attained a certain height they are orange-tinted; their epidermis is a bit withered; a slight pressure may cause them to open; meanwhile, the filaments on which

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with the wheat seed. The grape fruit, which interests us most because of the precious juice it contains, is, properly speaking, only a kind of envelope, a succulent capsule designed to retain the seed, and comparable to the glumes of wheat themselves.

they are borne elongate; the anthers, forced to project beyond the glumes, which immediately envelop them and the nascent kernel, come together and unite, because the kernel above which they extend no longer holds them apart; they extend into the light by way of the upper extremities of the two glumes that cover them: but the glumes, being well joined and closely applied one against the other by a flexible movement, closely enclose the stamens and the berry; in rising, the anthers ultimately succeed in imperceptibly pushing aside the two glumes, slightly exposing their tips to the light; the [anthers] come out, but in doing so, they undergo a slight pressure, which cracks them and causes them to emit their dust [pollen] that falls directly on the pistils of the embryo and on the nap surmounting its head.

Could I observe this admirable mechanism without being convinced that the nascent seed [ovule] depended on a sort of perfection of the stamen dust [pollen]; and that the liquid contained by the [pollen grains] was designed to enter the immature kernel, since, after a few days following the fall of the dust on the pistils and the tuft, it is no longer visible there and must have been dissolved?

Could I observe that the bunted heads became noticeable and abundant at flowering time, up to this time appearing healthy and whole; that the behavior of the stamens is interrupted; that the anthers are open, withered, and empty; that the kernels by degrees soon blast—could I consider these different effects in their ensemble without believing that there is a marked relation between the alteration of these nascent kernels and the moment when they should be fecundated? Was it possible, in short, to observe a prodigious number of insects on the wheat; find there their excreta and slough; see there their progeny, which they never leave except in places where the young insect, unable to move away, may find subtenance; was it possible, I say, to consider these little animals, making their common sojourn in the heads of wheat, without suspecting them of being the principal cause of some depredation and of destroying, in order to obtain food, the very place that had served them as a retreat.

I noted particularly the interrupted rôle of the stamens and the failure of fecundation consequent upon the damage they [the insects] caused in the bunted kernels, although

this defect was the only untoward condition I observed there. But I was strongly led to believe that the insects, living in great numbers in the wheat heads, and abundantly multiplying there, must cause a derangement in some part of the plant, injure it there, regardless of its nature, and pass with some probability as the remote cause of three major diseases that we shall consider.

I have already stated that the first thing by which I was impressed, in observing the shooting wheat plants, was the inverse rolling of the leaves and the malformation of the stems. It did not seem astonishing to me that the black insects, not yet finding the tender food that the immature heads must furnish them, attach themselves, while waiting, to the then very delicate culms and leaves; that they deranged their texture, and that the stems they would have attacked became misshapen from the fact that that side of the stem damaged by the insects makes less growth than the side they had not touched. This circumstance appealed to me: (1) As the consequence of a derangement within the imperceptible canals that conduct the sap to the head. (2) This point alone gave me occasion to consider the theory advanced by Mr. Wolf, to the effect that these false grains become blighted and die because their malformed vessels stop within themselves, as Mr. Wolf says, the flow of the nutrient juice and become a cause of disease.

I went farther, in suspecting that the aborted grains were the result of the more or less evident derangement that the black insects had been able to produce in the young stems. Because of these same animals [insects] I suspected also some evidence of damage in the smutted wheat plants. It seemed to me that they had been able to alter the tender and delicate epidermis of the nascent grain, of which, indeed, nothing remained in the smutted plants: I surmised from that that the farinaceous juice, being neither retained any longer by its enveloping membrane nor augmented through the base of the grain, finally dried, blackened, and became an impalpable dust.

The black dust of the smutted heads, viewed through a microscope, appears to be of the same nature as that of the bunt balls; the particles are simply smaller because the grain whence they come has not taken on so much growth as has the bunted kernel. The particles of the dust from the

Microsc. observ. p. 103.

smutted grains as well as those of the bunt balls are of equal size and perfectly round. I have not at all been able to agree here with Mr. Needham, either as to the primary cause of the dust of bunted kernels, as well as that of the loose-smutted grains, or as to the characteristic shape of the dust particles. Here is what he says: "Smut is a disease of wheat the farinaceous internal matter of which it destroys, and introduces in its stead a foreign material that blemishes and blackens the grain, at least externally. This material is a black and very fine dust but its particles, when seen under a microscope, are not of uniform shape." The English naturalist's next statement relates to wheat affected by ergot.

I shall not pause to show how Mr. Needham appeared to depart from the natural order in contending that in the disease called smut the farinaceous substance is replaced by a foreign material; I shall assert only that, by means of an excellent microscope, the dust particles of both bunted and loose-smutted grains appeared to me to be of uniform type. I shall state, moreover, that Mr. Needham's observations inspired me to examine them even more minutely than I otherwise, perhaps, would have done; and that I have always found them invariably of the same shape.

But how did this uniformity of particles in kernels reduced to a black dust come about whilst the flour produced by sound grain consists of quite unequal particles?

Perhaps one might reply that the farinaceous juice is composed of a multitude of small globular particles, whether they subdivide and become rounded just as one finds them or whether they are formed thus and become rounded in passing through the canals the structure of which is cellular and where, so to speak, they take shape: perhaps one might dare to venture the further opinion that this sap, composed of globules, once it reaches the interior of the grain, retains the form of its particles until the liquid from the staminate dust [pollen] impregnates the grain, makes contact with the particles of the farinaceous juice, and causes them to grow unequally; while the particles of the farinaceous juice of the bunted kernels or of the loose-smutted grains do not change in form because they do not share in the fecundating principle and contain nothing of the liquid contents of the stamen dust.

I fully admit that I am quite far from adopting unquali-

fiedly my own surmises and from going to the bottom of a theory followed in detail in which I disclose the manner in which insects may damage the wheat plant and cause the diseases to which it is subject; but I have not been able to abstain from entertaining a new idea, intimately related to my subject and derived, so to speak, from my observations. I have long adhered to it, especially in taking note of the fact that every day, under our eyes, plants damaged by insects grow only indifferently, or show some singular abnormality, or perish outright. I here present a fact that, in itself, at first contributed but little toward leading me to accept the insect hypothesis, because of its analogy with those insects presented on the diseased wheat plants: it is excerpted from the Natural History of the Barbados Islands.

Mr. Griffith Hughes, Member of the Royal Society of London, and author of that Natural History, says that sugar cane is subject to a kind of smut. He asserts that this disease is caused by swarms of little insects, which, when young, are scarcely perceptible; that these little animals, whose food is the juice of the canes, wound the still tender shoots, occasion there some little protuberances, derange the movement of the sap, and sometimes kill the plants. He says that the beginnings of this disease are difficult to recognize. He adds that smut is a periodic malady and that a canton, in which it has been observed one year, is affected by it the next year. This led him to presume that the insects deposited their eggs in the places frequented by them and made provision there for a numerous progeny.

Mr. Griffith [Hughes] believed that in attributing smut to insects one might explain very easily the peculiarities observable in this disease. I thought as he did and, to return to our subject, I did not sense the difficulty of reconciling the insect hypothesis as a cause of disease in wheat with the different observations I had given as principles and that have served me as an established basis for the discussion of all the opinions I have reported.

Before proposing with confidence any remedies against the major diseases of wheat, it would be necessary to understand thoroughly their fundamental cause, to observe it accurately; moreover, it would be necessary that this cause be of a nature to be foreseen by men and prevented by every

precaution that prudence and genius, aided by observation, may suggest to them.

I have already said that I am far from believing that I have touched the decisive point in attributing to insects the blast of the seed. The explanations I have given are not without obscurity, even though taken in their natural sequence; and I feel that it would be just to require of me more detailed, more positive proofs. Moreover, my hypothesis, which contains nothing contrary to the facts I have recorded, might not be in accord with other facts that I have recorded, might not agree with certain other facts that have escaped me. Possibly we shall conclude by considering the black dust of the bunted grains as the end product of some hidden internal contamination, differing little from that that one observes in Lycoperdon or the puffball, the intact envelope of which encloses a black powder, which, viewed under the microscope, presents an infinitude of round and coequal particles. One may, perhaps, be able to confirm this idea in recalling what travellers have said about the famous Apples of Sodom,[2] which, with a fresh, beautifully red skin, were inwardly like Lycoperdon.

Whatever it may be, I shall terminate here my first observations on the diseases of wheat. I shall pass without mention everything that I prescribed for the husbandmen, on account of the insect hypothesis, in order to prevent the diseases of wheat, or at least to lessen their effect. The precautions that I indicated had this advantage that they could be observed without their entailing more care or putting the farmer to greater expense. I even insisted on certain precautions that careful farmers never fail to observe, and I furnished, so to speak, only an hypothesis into which fitted conveniently methods employed to prevent the diseases of wheat.

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[2] One may see near the Dead Sea, a day's journey from the mouth of the Jordan' and on the west side, many of these trees of Sodom of which the writers of antiquity make mention. They are the height of fig trees and their wood resembles that of the fig tree. Their leaves approach the verdure and form of those of the walnut tree and their fruit is similar to that of big lemons, of which they have the shape and color but possess neither the firmness nor the quality: its beauty tempts and attracts the hand, but on seizing and pressing it, it yields and appears to be empty, like a sponge full of air. Foulcher de Chartres, who in his day also visited this country, speaks of these trees in this manner: "I saw there," he said, "something like apples in trees, which, on breaking the skin, I found only black and powdery within."

Flavius Joseph tells also of these same fruits of Sodom: He says that they appear good to eat, but that they crumble to a powder on being touched.

Voyage nouveau de la Terre Sainte, livre 4e. page 380.

Guerre des Juifs contre les Rom., livre 4e. Chap. 27.

Vertitur in cariem viridi sub cortice succus

It is well known that, being little satisfied with my first disclosures on the cause of the diseases of wheat, I felt more than ever compelled to investigate this interesting subject and neglect nothing that experience might teach me.

Although there was every appearance that the fundamental primary cause of the disease of wheat that constitutes the principal thesis of this memoir depends neither on the nature of the soil in which the seed is sown nor on the kind of manure with which the soil is enriched, nor on the time of seeding, nor on the particular kind of seed sown; although I am much inclined to believe that the most common preparation of the seed, though helpful, does not go straight to the root of the trouble; I desired to place myself beyond reproach; I sought only to put myself in a position of speaking solely from the facts, should I yet have occasion to explain myself on this subject; and I have resolved to employ experiments as much as possible in making deductions.

Consequently, last October, I chose an undisturbed plot of ground, suitable for wheat, an area 540 feet long by 24 feet wide. I divided my plot crosswise and made 5 equal parts, each of which was 108 feet long. The first of these parts had been manured with pigeon droppings; the second with the freshest sheep manure I could find; the third with night soil; the fourth with the manure of horses and mules; the fifth was not manured; after some tillage, the seed was sown, just as it was, after a year's storage. I gave the entire tract 4 ordinary cultivations: it received a fifth, of a kind, at the moment of planting, which was done by means of the hoe.

As my plan was to employ seed from different cantons, to select the best and the poorest, vary the preparations, and make my sowings at different times, I subdivided each fifth part of my entire ground into 6 equal parts each consisting of 4 beds, each bed being 18 ft. x 5 ft.; the remaining 4 feet of the total width of the tract was distributed among the beds and served as alley ways. This plan of subdivision gave me 24 beds in each fifth part of my ground, a total of 120.

I did my seeding on 6 different days: October 16, 22, and 27 and November 3, 10, and 22, 1751.

I stated that my planting was done with a hoe. This

easily managed implement made it possible for me to sow in each bed as many rows of wheat as I wished; I sowed 6 in each, on the first, the third, and the fifth days; I sowed only 5 on the remaining 3 days: thus the first rows were 10 inches apart and the others, 12. The furrows made by the hoe were hardly 3 inches deep.

Each seeding day, I endeavored to sow 4 beds of each fifth part of my ground. Thus, every day, where I sowed, I worked over each kind of manure, and the day's end found 20 beds done.

Each time that I went out to sow, 4 little ticketed bags were carried out, the first of which contained wheat treated with simple lime or lime mixed with saltpeter, according to the day of seeding; the second bag contained nontreated wheat; the third contained wheat treated with lime and sea salt; and the fourth, wheat blackened with the dust of bunted kernels.

I have always taken care that the treated grain, as well as the nontreated, came from hand-picked bundles from which all bunted and loose-smut heads had been removed. The purposely smutted wheat did not require the same precautionary measures. I have frequently collected it on the market from among the poor, shrivelled, or tarnished wheat. The good seed that I used in the 6 sowings came from different cantons and was furnished me by farmers with whom the grain was popular.

In sowing my ground, I used only 12 pints of seed, or thereabouts; which amount, after making all deductions, represented about half the quantity of seed used by local farmers in seeding an equally large area of plowed ground.

Since my two neighbors did their seeding at the same time I did mine, I shall have opportunity to examine something that has long puzzled me; that is to say, the evident loss, in the ground, of a part of the seed, which the plow buries too deep; since, at the rate of 4 and sometimes 5 bushels of wheat per arpent, broadcast, one commonly gets a return of not more than 20 to 25, and, in good years, 30 bushels per arpent.

In addition to the sowings made in the open field of which I have just spoken, I prepared some beds in a garden in which I sowed wheat. Certain of these beds were left in their natural condition. Others were fertilized with poultry

droppings or with hog manure. I even made one bed, 18 ft. x 3½ ft., out of woodland moss and without communication with the soil. I sowed some wheat in this moss and it is now (February 15) almost as advanced as my other sowings, although it was not sown until December 9.

One may see from all this detail that I shall be able to depend a little on the observations that the wheat of my little tract and that of my different beds will enable me to make; and that from the large number of combinations resulting from my sowings, some light may be shed and will find place in this memoir should I be so fortunate as to have some of it impinge upon me.

SUPPLEMENT

THE experiments that a man, himself, conceives and performs have something to their advantage. They are valuable to him who performs them, hence he follows them closely. These experiments, with which he becomes very familiar, strike him from the different aspects in which he may view them and afford him opportunity to multiply his observations by increasingly stimulating his curiosity. The wheat plants produced by the plot, the distribution of which we have noted, having not yet acquired their full growth, I am unable now to give the result of my sowings. Already, I see several facts to put down concerning the major diseases of wheat, others to develop, some reflections to be added.

I am pleased that the Academy will be agreeable to accept a supplement in which these facts will be presented in greater number and in a manner more detailed than in my memoir. They will enable me to rectify my ideas on an important point and will make more evident than ever the inadequacy of all that has hitherto been said in an attempt to establish the true cause of the diseases of wheat.

I have observed these diseases in their initial development in so far as it has been possible. I have caught the rachitic condition in its incipiency, so to speak, at a time when the wheat stem begins to form; I have followed the progress of this disease throughout its entire development; I have recognized bunted wheat plants several days in advance of their flowering; I have been fortunate enough to find some heads destined to become loose smut, although

tightly enclosed within their sheaths; I have noticed that such heads, after having begun to show a kind of moldiness, rot insensibly and finish by changing into dust. But, let us present the facts; they will constitute in themselves the series of my observations and will serve me in giving an historical account of them.

Facts concerning blasted wheat plants

I. Wheat heads in blasted condition ordinarily occur on stunted culms of a bluish hue, the leaves of which are curled.

II. The stunted condition of the culms obtains from the time they are only 3 or 4 inches tall.

III. When the stunted culms are short they are flimsy and of a yellowish color; their leaves are of the same color and a bit curled and withered.

IV. As the stunted culms grow, they assume a greenish color and ultimately acquire that characteristic bluish hue peculiar to them: their leaves, always remaining curled, also become bluish and never possess the vigor and strength of healthy leaves.

V. The stunted condition is not one peculiar to poor, impoverished land; I have observed it on good soil in the midst of the finest wheat plants; and sometimes I have vainly sought it where the wheat was very thin and appeared to be in poor condition.

VI. The roots of stunted plants have seemed to me to be a bit altered: Many of them were woody; such roots were not so completely and generally covered, as are those of healthy plants, with that spongy, velvety cork the function of which is without doubt the moistening of the ligneous part.

VII. Rarely does the same stunted wheat plant bear both completely sound and wholly blasted heads; when that happens, the good heads are borne by erect culms whose leaves are but little curled.

VIII. The kernels from stunted plants are like young peas and show one, two, or three very distinct apices.

IX. The pea-like or blasted kernels that ordinarily occur on stunted and curly-leaf stems, are sometimes found along with bunted kernels in the same head, borne on erect culms whose leaves show no curling.

X. A stunted culm sometimes bears sound kernels and pea-like kernels in the same head.

XI. The blasted kernels are very weakly attached at the base of the glumes; they become easily detached when the glumes are pushed aside for their observation.

XII. One very occasionally finds a blasted kernel retaining one or two of its stamens; I have never seen any that bore all three.

XIII. The blight of the kernels is sometimes entirely consummated before the head is out of the boot.

XIV. Occasionally, when the heads of stunted plants have been exposed to the air, the glumes begin to fade and the blasted kernels blacken and become dry.

XV. One sometimes finds certain stems of wheat that, although stunted, are quite erect, very tall, and bear curled leaves only from the 3rd and 4th nodes. The heads contain blasted kernels in one, two, or three spikelets; the blasted kernels being enclosed by glumes containing a white grain accompanied by one or two yellow stamens; the white grains bearing three very green stamens and apparently destined to function normally.

Facts concerning wheat affected with smut [*loose smut*]

I. Smut [loose smut] is a disease that commonly extends to all of the heads of a given plant.

II. It sometimes happens that a smutted head is produced by a plant that puts out several healthy heads; or that a given plant produces several smutted heads and a single healthy one; or that one and the same plant may produce several smutted heads and one bunted head. A smutted head may sometimes be only partly smutted, the remaining portion of the head bearing either bunt balls or sound kernels. In a single head there are sometimes only one or two smutted kernels.

III. It is easy to detect a smutted head, even though it be tightly enclosed by the boot and exhibit no outward signs of its presence. Smut begins with a sort of mouldiness observable on the still green head. The head gradually blanches; some black spots form on the glumes; little by little they break down, retaining always a certain degree of humidity: the entire head finally breaks down, dries out, and becomes a black dust, with the exception of the rachis, the awns, and some remnants of the glumes, which dry up without being affected.

IV. When the head begins to show signs of smut it may

be two inches long or thereabouts; the nascent kernel is then hardly noticeable.

V. It is seldom that a smutted head contains either sound or bunted kernels; I have yet found no smutted head that bore aborted [blighted] kernels.

VI. The roots of wheat plants affected by smut seem to have been altered, the ligneous portion being deprived of its cortex in some places. These roots bear few root hairs: otherwise, they have the same consistency and the same elasticity as those of healthy plants.

VII. The upper part of the stem of smutted plants, from half to three-fourths of an inch beneath the head, is commonly not very straight and presents a slight, more or less evident sinuosity; if one squeezes the stem at this point, it resists and scarcely yields, at all; if, at two or three lines beneath the head, one cuts this same stem horizontally it will be found almost entirely filled with a medullary substance, one will see in the middle a tiny cavity: if, instead, one presses with the nail this same part of healthy stems one will feel only a little resistance. The parts come together easily; also, if this part of the stem is cut horizontally, it presents a lumen proportionate to its size which joins up with that obtaining throughout the culm: it appears that at the top of the stem of the smutted plant there is a sort of obstruction.

VIII. When from a wheat plant there arises a smutted culm and when from this same stem there arises another that is totally dependent on it, this secondary stem always is affected with smut. This observation has been true also of both blighted and bunted plants.

Facts concerning bunted wheat plants

I. Ordinarily, all the heads of a bunted plant are entirely bunted; in the same plant, as many as eight bunted heads have been found.

II. Some bunted heads may appear among healthy ones borne by the same plant.

III. One sometimes sees heads that are partly bunted and partly sound.

IV. A head attacked by bunt sometimes presents several gradations of this disease when caught in its incipiency. The head contains some apparently sound, whitish kernels

each accompanied by its three green stamens; some kernels that are slightly blotched with green of which one or two of the three stamens are yellow-tinted; other kernels that are almost wholly green, the stamens of which are nearly all yellow; and finally some kernels that are entirely green and in their last stages of disease, the stamens of which are quite yellow. It sometimes happens that in this sort of head, where bunt has produced a more or less marked effect one finds a kernel without any defect and accompanied by three green stamens, although this sound kernel may be in a spikelet along with an entirely bunted kernel, the stamens of which are yellow.

V. Bunted heads never blossom, or, if some of the kernels of such a head bears stamens projecting above them, they blight and are of none effect.

VI. When from the same plant there grow several bunted heads, the presence of bunt is noticeable in the youngest heads, even though still in the boot; the stamens are yellow and drooping; the embryo assumes here and there a greenish tint.

VII. The heads attacked by bunt, even by the time the stem is full-grown, are soft and never possess the consistency of sound heads. The leaves of the stem bearing such heads are dark green and spotted with yellow.

VIII. In general, in all the bunted heads the three anthers, although collapsed and supported by filaments that, themselves, are very slender and withered, are disposed about the kernel precisely as they should be; their triangular position is accurately observed.

IX. Bunt balls are sometimes found in otherwise smutted heads; or are mixed in the same head with blighted grains.

X. The roots of bunted wheat plants, when one pulls up a stem, do not seem to have the same firmness, the same strength, the same hairiness, and so many minute branches as do those of healthy plants; it seems that the roots of bunted plants have undergone some change.

XI. The glumes of bunted heads bleach and become dry when exposed to the air for some time. The glumes of healthy plants of the same age do not fade until they begin to mature.

M. Duhamel seems persuaded that smut and bunt are

virtually one and the same disease that finally comes to fruition as so many smutted heads. To the reasons he advances in support of his opinion we might add one that has escaped him, one that is, nevertheless, striking enough to give some likelihood to his concept; that is to say, *it sometimes happens that one and the same plant is affected with bunt and with smut; that one and the same head may be stricken by both diseases together.*

M. Duhamel has not referred to the disease described in the foregoing under the name of blighted wheat: thus he has not been able to explain the connection and the relation one would be tempted to picture between this disease and the two others he observed. If the statement I have added to that of M. Duhamel, in order to establish a sort of analogy between the occurence of smut and that of bunt, were plausible, one might use it with equal justification in maintaining that the blight of wheat may be obscurely related to smut and bunt, since the bunted heads sometimes contain blighted grains and the leaves of a plant attacked by bunt sometimes are curled, a frequently observed and common indication of blight. Bunt balls are sometimes found in an otherwise smutted head of wheat; blighted kernels are sometimes found among bunted kernels in the same head; the connection is obvious; blight depends on bunt; bunt depends on smut; I omit here several particulars common to the three diseases of wheat; they appear in the perusal of facts that I have assembled. However, I would not dare to consider the connection, the obscure identity of these three diseases, as being very constant. There are some varieties that seem assignable to wholly distinct classes; and I am today very sure, through repeated experiments, that whatever factor may be an abundant source of bunt in wheat is not at all a cause of either blight or smut.

M. Duhamel has clearly shown how vain it is to seek the cause of the major diseases of wheat in the influence of mists, in the failure of fecundation, in the shock of bright sunlight impinging on the heads while they are imbibing water, and in the excessive wetness of the seed bed.

I shall not stop to bring together more detailed facts than those I now give to disprove the theories that several naturalists have advanced. It is plain that they are not supported by repeated observations and that they have by

no means brought together the extremely numerous and varied effects that an experimental consideration of the subject discloses. It was, however, an excellent route by which to arrive at the determination of a cause that satisfied all or that at least left but few obscurities. I believe I should only note the fact that there is no likelihood whatever that smut and bunt are caused, as Mr. Wolf asserts, by the anomaly of kernels in which the sap has been stopped and has stagnated. From several of my observations it has been decided that the bunted kernel possesses its natural shape before it becomes diseased, that it retains its shape quite well after completing its development, and that the smutted head, when it begins to break down, contains scarcely perceptible kernels and shows nothing by way of a malformation. Mr. Wolf has considered barley only, and upon this plant has based his conclusions for all those plants that are subject to bunt and smut.

It will, moreover, be worth while to apprize you of the fact that I have to-day some new evidence to show that the great abundance of water with which the culms of wheat are filled is the cause of neither smut nor bunt. In order to be more certain, Mr. Tull's experiment has this year been repeated under my observation by an attentive and exact person. One wheat plant produced by a single kernel was watered three times per day and was nearly flooded with water. I had transplanted this plant in the same earthen pot as that whose dimensions have been mentioned elsewhere in my memoir. This plant, which grew prodigiously, is already bearing several fine heads and I anticipate seeing very few if any bunted heads among them.[3] I shall add here that the wheat grown in that bed of pure moss referred to towards the close of my memoir is now a thing of rare beauty. It has produced a large number of very healthy heads some of which are as much as six inches in length. Throughout the whole extent of this bed I have thus far observed only two bunted heads.[4]

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[3] This plant has produced 89 culms, the majority of which are three to four feet high. Although among a large number of very beautiful heads there may be several poor ones, others that may be slender and shrunken, none, however, is affected with bunt and none manifests any blight or loose smut.

[4] I made this observation on the 18th of June, 1752. It will readily be seen that if I noticed then only very few bunted heads in this moss bed it was because the disease was slow in manifesting itself in the part of this bed where it should pre-

Even though one remain persuaded that certain fertilizers, certain soils, in themselves, contribute to the disease of wheat and render disease more general, one will at least admit that they are not the primary cause, since wheat plants come up from the pure, washed moss and, with every possible attention, only to be found affected by the most important of these diseases. On the other hand, if excessive moisture is the source of the diseases of wheat one should frequently notice them in that moisture that has produced the moss, since it is nourished only by water and languishes when watering is neglected.

I would hardly be able to state with certainty at this time that the nature of fertilizers, considered solely from the standpoint of animal excreta, lack of treatment of otherwise well-selected seed, the nature of the soil, earliness or lateness of sowing, condition of the weather during seeding; I would, I say, hardly be able to assert with certainty that all these exert no influence on the diseases of wheat and disclose facts that always leave an observer in doubt. After harvest, I shall have occasion to return to this subject and enter into such detail as it is now impossible to give with certainty.

I would not, however, know how to pass in silence a matter that will be a part of this account; i.e., that in examining the different beds, the plan of which has been laid before the Academy for examination, I was surprised to note how all those in which I sowed defective seed or seed purposely blackened with the dust from bunted kernels, contained a great quantity of bunted heads, notwithstanding the facts that these beds were fertilized with different manures, that among them there were six that had received but one kind, and that the seed sown there varied according to the canton whence it had been obtained and the time it was put into the soil.

Last year I did not know exactly where the black insects, considered in my memoir, laid their eggs. I have but recently discovered that these almost imperceptible eggs are deposited along the rachis of the head, at the base of the pedicels supporting the spikelets: they are easily made

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dominate, and it attacked there in the course of time a large number of heads that subsequently showed the same symptoms and precisely the same developments as did the wheat plants that drew their moisture from the soil.

out by means of a magnifying glass: ordinarily, they number two or three. I have seen as many as eight at the base of one pedicel; they are much more commonly found on the good than on the bad heads.

I have stated that the incidence of bunt might be occasioned by these same black insects and that I suspected them of damaging the stamens, thereby causing a failure of fecundation and a consequent deterioration of the ovule. Following some exact observations, I have retraced my steps; I have reflected on how several bunted heads were in that condition, although snugly enclosed within the boot and thereby inaccessible to insects. I have remarked that since the disposition of the stamens around the bunted kernels, sometimes collateral, sometimes intermediate, is precisely that fixed by nature, it necessarily must follow that the disease did not result from any external derangement. I felt that insects, however minute they might be, would, of necessity, leave more or less noticeable indications of their ravages around a very delicate kernel if they found there any nourishment, and that at least the stamens would not always be in their natural position. These reflections, in addition to several observations, which it is useless to recall, have caused me to conclude that both smut and bunt are wholly internal diseases; that when the head emerges from the boot, either the disease has attained already its full development or it will come to maturity little by little in the head exposed to the open air without any external contributing cause.

Sixth observation made on blighted wheat plants. Sixth made on smutted wheat plants. Tenth made on bunted wheat plants.

It is necessary then to seek a cause the effect of which is made manifest within the very interior of the culm, a cause that leads to a plausible explanation of all the capriciousness one observes in the three diseases in question.

In spite of a very exacting study on my part and an examination of wheat in the different stages of growth in which I have been able to secure it, I noted, in the light of some of my observations, no other cause of its diseases than an alteration within the roots whether through the many-times cited work of black insects or that of other creatures[5] unknown to me.

[5] I have sometimes discovered among the roots of wheat plants some insects that appeared to me to be a sort of plant louse. Once I saw them when they were quite small and in sufficient quantity to induce me to believe they had originated there. On the same roots where I saw them, there was a plant louse much larger than they

The little black insects that one finds associated with wheat as soon as the head is formed, live, according to every indication, and remain hidden, underground during the winter. With the first fine days of spring some of the insects begin to come out and spread to the most vigorous plants: I have seen them in the month of April. I would not depart from the natural order, if, after having noted a slight alteration in the roots of diseased wheat plants, I suspected that these animals obtained nourishment from the sap of the roots and that they, in slightly deranging the tissue, inoculate it with a sort of virus that diffuses throughout the plant and produces its major effect on the head. I find myself drawn to this idea by two apparently correct observations.

1. In examining wheat plants during the winter, I noticed several had died. I did not know why. It was only after pulling up some of them that I noticed the roots were injured and that the bad external condition, by which I was impressed, resulted from the alteration that the roots had undergone.

2. The stunted wheat plants are early recognized; on reaching a height of 3 or 4 inches they are yellow, wilted, and soft; their leaves are curled and withered. These same wheat plants, on resuming growth, become bright green and complete their growth by assuming a dark bluish green. One may assume that the stunted plants have suffered a considerable reaction in their roots during the winter, that they have put out new ones with the beginning of spring, which supplied them with a little vigor; but that in these same sick plants, being yet unable to find in these few new roots the resources of a completed growth, their culms and leaves have languished and their grain become blighted.

Although I have carefully washed a very large number of wheat plants attacked by bunt, or smut, or that were in a blighted condition; although I have examined by means of

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and, which, without doubt, was a female. These insects should be followed up, but that is a task. When one pulls up a wheat plant with all its roots, they are covered with soil and are all mixed up: it is difficult to observe the very small creatures by themselves hidden among the soil-covered roots and always exposed to destruction by the hands of the observer when he washes the roots in order to hunt for them. M. Réaumur, whom nothing escapes, says that there are some kinds of plant lice that attach themselves to the roots of various plants and feed on them. Among other roots he cites those of oats, which, as is known, is one of the plants subject to smut. Mém. sur les Insectes, Tom. 3, pag. 340.

a hand lens or with a microscope the parts of roots that seemed to me to be injured, I have seen nothing that fully satisfied me. Moreover, the damage that may have been done by such small insects as ours to the roots that have grown since the time when it was believed they were attacked, roots covered with a spongy cortex, pitted with an infinite number of pores, and provided with a kind of nap, this damage may well be of such nature as to escape the most painstaking search.

I have noted above that all the beds of my plot in which I sowed bunt-inoculated seed without any preliminary washing; I have, I say, noted that these beds, thirty in number, contain much [smut] although those alongside have very little or none at all. Might not, then, the diseases, smut and bunt, be heritable? The observations of M. Duhamel, the experiments he reports, do not entirely eliminate this concept. Do the bunted kernels contain some virus? Might not their black dust be impregnated therewith to the point of communicating it to the seed? Might not this malign dust produce its effect on the good seed by penetrating it with its virus when the seed begins to swell in the ground and when its germ begins to grow? Are not the wheat plants grown from smutted but subsequently washed seed less likely to engender smut than those grown from seed of the same sort that had not been washed? Might not this be because the washing removes the black dust from the seed and thereby prevents the evil effects that it, the dust, would produce?

Whatever it may be, this considerable quantity of bunted heads that I have noticed only in those beds sown to inoculated seed, deserves special attention. I shall not fail this year to sow again some smutted seed alongside other thoroughly clean seed and select the best heads. It would be helpful if several people would volunteer to carry out the same experiment in several different cantons. Perhaps it would result in our knowing to a certainty whether the dust of the bunt balls is a germ of disease; whether the smut disease is hereditary.

EXPERIMENTS
AND
CONSIDERATIONS
RELATIVE TO THE DISSERTATION
ON THE CAUSE
Of the Blackening and Destruction of Wheat Grains in the Head and on the Methods of Preventing these Unfortunate Circumstances

SECOND PART. 1751, 1752.

After the details into which I entered in the Memoir, which I have had the honor of presenting to the Academy, and in the yet more detailed supplement I have added thereto; after these details on the different diseases of wheat, on the special classes to which they should be consigned, and on the specific symptoms that characterize them, I should be permitted to summarize the three principal diseases and the names I have given them.

I have signified as blighted those wheat plants whose culms are ordinarily a little twisted and more or less stunted, bearing blue-green leaves that are somewhat curled, green kernels terminating in one, two, or three minute bosses and quite similar to a pea in the early period of its development within the pod.

I have given the name *blasted* [smutted] wheat to those plants of which many of the heads appear to have been destroyed by fire; in fact, there appears to be left of such an attacked head only a black powder. Sometimes, little white threads escape from the degenerate mass and always a kind of central column along which the grains were arranged.

I have finally designated under the name *bunted*[1] wheat those plants the disease of which should properly be made the object of our investigation; those plants, the heads of

[1] This term, as well as that of bunt, will so often recur in what follows that I shall sometimes employ in their stead the term smut, already in use, although too general in its application to characterize clearly the disease in question.

which, unfortunately too common, do not blossom, although apparently well-formed and equipped with stamens. Their kernels, deteriorating gradually without losing their natural shape, finally become, internally, a mass of soft, blackish dust of unbearable odor.

Although the experiments I made in 1752-53 throw the most light on the disease of bunted wheat, which will be the principal subject for consideration in this supplement, I have, nevertheless, thought it would be well to review what I did in 1751-52. Aside from special facts brought out in those experiments, of which it is well to take account, it will be perceived that they shed the first light to guide me in subsequent experiments. It will be seen how an effect that I had not anticipated, even though I had worked to bring it to pass, disclosed to me a cause of disease, which, until then, had not been suspected.[2] This cause, obvious as it is, did not appear to be of any consequence to wheat, being, as it seemed, out of all proportion to the marked effects we shall subsequently witness. The reader, in considering the essential objective to which I applied myself, will be the better able to judge of the progress and of the correctness of my observations.

See project 1.

At the end of my memoir there was an account of the preparations I made in 1751 incident to the experiments concerning which I shall have something to say the following year. It has been observed that the plot on which I carried on my work had been under cultivation and was one of the best for wheat culture in the canton. It has been noted also that this plot was 540 feet long by 24 feet wide, and that it was divided lengthwise into 5 equal parts; that the first part was fertilized with piegeon droppings, the 2nd with very rich sheep manure, the third with night soil, the fourth with horse and mule manure, and the fifth received no fertilizer of any kind.

Care was taken that each fifth part of the plot, 108 feet

[2] It is true that M. Duhamel has had some slight suspicions about the possibility of bunt being a hereditary disease. Some authors, without reference to their precise experiments, have, in general, counselled against sowing *tipped* wheat, *soiled* wheat. But one should observe here that the really useful advice of these authors and the opinion of M. Duhamel share the same principle, namely, the supposition that the germ of the disease resides in the grain itself; that it is essentially internal. It does not appear that they suspected (which is the essential point he needed to grasp) the cause of the malady to be external to the grain and dependent on some particles of dust distributed over its surface membrane.

long, was divided into six equal parts, each composed of four beds 18 feet long by five wide. The four remaining feet, extending all the way across, were left to serve as alley ways between the beds and as plot borders.

It has been noted that my seeding was done at different times, i.e., on six different days, separated by intervals. These seeding dates were the 16th, 22nd, and 27th of October and the 3rd, 10th, and 22nd of November. It also has been observed that I sowed the seed in rows at a depth of two or three inches, following somewhat the method of M. Duhamel. Care was taken to place the rows just 10 inches apart. It has been seen, too, that in planting 20 beds each day I covered those that had received each kind of fertilizer and also the unfertilized portion; that the first kind of seed I used was either dirty, poor, and shriveled wheat, obtained on the market and in different sacks, or wheat that I had purposely smutted with the dust from bunt balls; the three remaining sorts of wheat employed in my seeding consisted of choice seed obtained from four different cantons and treated with lime and a saturated aqueous solution of common salt or left in its natural state or treated merely with lime or jointly with a saturated solution of nitre.

According to the order observed in sowing, the dusted or purposely smutted wheat was sown always in the first row; the seed treated with lime and salt, in the second; the non-treated seed, in the third; and that treated merely with lime or with lime in combination with nitre, in the fourth row. This precaution was useful to me; each kind of seed thus planted in 30 consecutive beds, presents a singular prospect to those who observe them but have no idea of the cause, and, even to me, who has commenced to have some inkling of it.

I pass without comment the less important observations I had occasion to make on the condition of my beds by the close of 1751, and at the beginning of 1752. It really was not until the early part of April that anything offered worth noting in my journal; even the first observations I shall here narrate will offer nothing of much interest.

On the 9th of April, 1752, I noticed that several wheat plants were dead, while others were very small, spindling, and of a sorry aspect. I pulled up some of them and found them partly rotten. It seemed to me that they had suffered

from having been buried a little too deep, either through faulty seeding or because they had been covered through the removal of ground underneath by mice or moles.

On the 11th and 12th of April I gave my beds a light cultivation and pulled up all the plants that had grown between the rows during the winter. This work gave the wheat plants new vigor.

The 22nd, everything was going well. The first beds of the portion of ground fertilized with night soil were in a very flourishing condition and resembled most excellent patches of rye. I gave particular attention to a large number of culms the leaves of which were frayed; I pulled up five or six of them, but saw nothing unusual about them beyond this slight derangement in leaf tissue; I, however, selected one of these culms with torn leaves and noted some yellow spots. I opened the sheath of a leaf and found there a greenish and minced material that looked as though it might have been intended for some kind of sauce. I examined this stem with a magnifier and this green material appeared to me to be a yet enclosed leaf that some insect had penetrated and had partly broken and eaten: the base of the still very delicate stem was not at all damaged. The little nodes, destined to be lifted one above the other, could be distinguished and at the extremity of the last node could be seen the well-formed head as shapely as the point of a needle and perfectly sound.

One of the first things to attract my attention when the wheat began to shoot was a sort of fluting, very noticeable on a large number of leaves. I was, at the time, much occupied with the insect hypothesis, believing it adequate to account for the grain diseases. At first, I believed that this leaf alteration could be attributed to insects; but, on a more searching examination, I found that this fluting was due to a pronounced constriction of the leaves while they were yet rolled up in the sheath. This sheath, without doubt, not opening so quickly as the growth of its enclosed leaf demanded, caused the leaf to fold back on itself and to assume somewhat the shape of a screw; and, with the development of this leaf, a part of it became lifted in a very regular manner, while the other part, just as regularly, formed a little hollow. I sometimes found leaves still rolled up and thus crowded in the sheath, which showed this sort of fluting

on being released. This derangement of the leaves, non-essential in itself, might be attributed to the force exerted by the plant's growth; in fact, I have noticed that it was common in vigorous wheat plants and quite rare in poor and languishing ones.

Here was the occasion, in order to follow the order of my observations, for me to note down some of the details relative to the three diseases important to wheat; to note their initial, though scarcely perceptible, beginnings; to follow their daily progress; and to characterize them by everything that individualized them, and to assign them to separate classes. This was the occasion to speak more positively about the insects mentioned in my memoir, those animals belonging to the order *Staphylinidae*, which I followed so unremittingly and so uselessly, at least from the point of view from which I considered them: this was the time to note the error into which I had fallen in suspecting them of damaging the wheat grains and of being indirectly a cause of bunt. It was, in short, the moment to recall the observations I had made on the roots of sick wheat plants, being attracted to them by the change that I thought I observed in several among them and being no longer able to find the origin of the disease in the changed condition of the head. But these matters have been presented in my first supplement, and I now shall pass directly to a matter of greater interest in my experiments and that subsequently occupied my entire attention.

It has been noted in the foregoing that in the first row of my plot comprising 30 beds I had sown only bunt-inoculated wheat. I noticed, toward the middle of June, 1752, that there was a considerable number of bunted heads among those 30 beds, while I observed almost none among those of the three other rows. Shortly after, the disease became so manifest in these 30 beds that I finally perceived what I suspected might be the cause of an injury confined to a single row and concerning which I sought to make myself more certain by scrupulous attention to the least thing that might tend to confirm my suspicion.

In carefully examining each of the 30 beds attacked by the disease, I observed that there was more of it where the injury was more generally present than in other beds. I fixed my attention at first on the six beds fertilized with

pigeon droppings (it is always a matter of the first row attacked by smut).

In the first of these beds there was nothing but bearded wheat. Half of the heads were found bunted. In the second, also occupied by bearded wheat, ⅗ of the heads were bunted. In the third, composed partly of bearded and partly of beardless wheat, ¾ of the heads were affected by the disease. In the fourth, devoted to a variety of wheat similar to that in the third bed, more than half of the heads were blasted. The fifth, containing only beardless wheat, was so infected that of 8 heads scarcely one could be found that was not bunted. The sixth bed, devoted solely to beardless wheat, was in as deplorable condition as was the fifth.

I examined just as carefully the beds of the first series in the other 4 divisions. The number of bunted heads was the same as in the first, in spite of the different fertilizers used in the second, third, and fourth divisions, and, although there was none used in the fifth division, this quantity of bunted heads, the same in each division, occurred exactly and for each individual bed, in the same proportion noted for those of the 1st division; that is to say, that in the first bed of the second, third, fourth, and fifth divisions half of the heads were bunted, as were those in the first bed of the first division; that in the second bed of the 4 last divisions, ⅗ of the heads were attacked by bunt, as was the case in the second bed of the first division; and so on for the rest. And this proportion was such that one could judge precisely of the condition of this or that bed of the first series in each division by the condition of the corresponding bed of the first division.

The beds of the second series, for which the seed had been selected and had been treated with lime and sea salt, showed but few bunted heads in the five divisions. It was only by hunting carefully that one could find them. I observed them without difficulty, although few, in the beds of the third series the seed of which had received no treatment. The fourth series was the one in which the disease was least apparent. The beds comprising it had been sown, as noted in the foregoing, to selected and treated seed, treated either with lime alone or with nitre and lime.

This 4th series, where the heads were sound, gave me opportunity to make an observation that is worth reporting.

The ground where I conducted the experiments the details of which I have given, is situated in the midst of a vast field that belongs to different individuals. The nature of the soil requires that it be carefully cultivated, and the same year of my experiments two of my neighbors sowed wheat there. The small portion of ground I occupied was separated from their fields by a mere furrow and I had every opportunity to make a comparison between the condition of their wheat and my own. My neighbor's wheat field, which bordered my first series of beds, was healthy, though the soil was poor; it is true, however, that this series contained almost no smutted heads. The wheat plants of the portion of the field bordering on my fourth series were in a deplorable condition and in great part smutted, whilst those in my fourth series presented a delightful aspect, as much because of the beauty of the heads as because of their healthy and vigorous condition.

I now come to the observation I had occasion to make in examining the wheat plants of this fourth series and those of the neighboring field. This field was sown only after my beds, themselves, were sown. The farmer threw some grains of wheat onto the borders of my plot. He kept close to one side in guiding the plow and covered at the margin of my plots a few of the seeds he had thrown there. The seed germinated there perfectly but carried the defect of the seed of which it was a part. Half of the heads coming from it were found to be bunted. They constituted a hit-and-miss lot of scattered plants alongside of my last rows of wheat; hence it was easy to recognize them as extraneous to the beds in which all was order and where the wheat was in rows, only.

In none of my beds did I notice any blighted heads. I found in them some loose smut, sometimes in one bed, sometimes in another. Those showing the most bunt contained no more heads attacked by loose smut than did those in which there was almost none.

The experiments, the results of which I give here, were made in an open field. It was at first necessary not to depart from the usual order: I did not, however, neglect to try some experiments in the city. In a little garden surrounded by buildings, I put in 4 plots 2 of which were fertilized with hen manure, another with refuse from hogs,

and the 4th was left in a normal garden-soil condition. My object was to determine whether, as some writers assert, these fertilizing agents, reputed to be very ardent, contribute to the production of bunt. In one or two plots where hen manure had been applied, I sowed some selected but non-treated seed; and in the three remaining plots I sowed some of the same sort of seed but treated with lime and sea salt.

With the exception of 5 or 6 heads of loose smut, as many in the beds fertilized with hen manure as in that to which hog refuse had been applied, I observed nothing abnormal. I sought everywhere in vain for some bunted heads: the fertilized plots were as free from the disease as were those where no manure had been applied.

It was otherwise with the plot of moss, mentioned in my Memoir in the course of the experiment I am here citing.

It may be recalled that the moss of which I made use had been got in the woods; that it was carefully sifted before receiving the grain; and had afterwards no contact with the ground, although placed in the earth. Following is the simple method I followed:

On the 9th of December, 1751, I had made a trench in a large well-aerated garden. This trench was 18 feet long, 3½ inches wide, and 9 inches deep. At the bottom of the trench, as well as along the sides, I took care to provide some little outlets to give egress to the surplus water.

I put the moss in this trench and pressed it down reasonably firm. When this bed of moss was finished, I divided it lengthwise into 4 equal parts, taking special care to mark the separations with some little sticks placed on the surface of the moss and attached by their ends to the borders of the plot. I sowed in the first part some seed blackened with bunt. In the second, some selected seed treated with lime and sea salt, in the third, some clean, nontreated seed, and in the fourth, some selected seed treated with nitre and lime. This sowing plan was the same as that I had followed in preparing the plots concerning which I first spoke, and I used for these moss plots a little of the same lot of seed I had left from my sixth and last sowing.

My seed, thus sown, was covered with a layer of moss 2 to 3 inches thick. At first, I sprinkled this plot with a quite considerable quantity of water in order to wet it down well. Thereafter, it was maintained exactly at a cer-

tain degree of moisture by watering once or twice a day, according to need, the watering being discontinued only at times when the plot was covered with snow or during hard freezes: In the latter case, I took care to cover the bed with a layer of straw. The seed, although sown late, germinated quite promptly. The young seedlings became strong in a short time and withstood the rigors of winter, although the surface layers of the moss were frozen at times. These plants shot up vigorously in the spring and followed in their development the progress of ordinary wheat. The heads, however, came to maturity only toward the end of August; some, even, produced on late culms, were gathered without being fully ripe. Aside from this slight imperfection there was nothing to concern me. I was interested only in whether the grain was sound or diseased.

I had the satisfaction of seeing this last experiment tally with those I have already reported. In the first division, where the seed had been blackened with the dust of bunt balls the disease was manifest in nearly all of the plants. Among others, one was found with eleven culms the heads of which were entirely bunted. The beginning of the second division contained a few bunt heads, either because some seeds had come up beyond the little stick that separated it from the second or because the disease had found it easier to progress in the moss and become contagious than had been commonly the case in ordinary soil. Whatever may have been the cause of this slight accident, at least it is certain that there were very few bunted heads in the second division; that they all were neighbors to the first division and seemed to belong to it; that the third and fourth divisions contained no bunt; that this part of the plot was excellent; that there could be noted there some plants bearing a large number of culms some of which bore heads measuring as much as 6 inches in length.

To the simple and purely historical account I here offer concerning my experiments, I shall add only a few comments. They are, themselves, presented in the results I have faithfully narrated at the conclusion of my different operations. The following observations will, therefore, be only a summary of those that contain certain important points and on which I already was informed.

In the large-scale experiments concerning which there

at first had been some discussion, it was observed that the wheat of the field adjoining my first series of plots was healthy, while that of this same series was bunted; and that the wheat of my fourth series was very healthy, while that of the neighboring field was damaged.

Let us not hesitate then to exclude mists as a cause of bunted wheat. They would produce their effect more universally if they really occasioned any such malady; and one would not see it exactly limited to this or that restricted area, since it would depend on a weather condition that ordinarily occupies a quite considerable area and is nearly always unequally distributed.

Let us not hesitate to regard the manures, the excreta of animals, as being incapable of causing bunt in the field where they are employed.

In the first experiments, the pigeon droppings, the sheep manure, the night soil, the horse and mule manure; in the second, the hen manure, the hog manure,—none of these fertilizers, as such, produced any evil effect; at least, none was evident. If, in certain parts of the nursery where these manures were applied, some smut did appear it is necessary to find some other cause if we are to discover the reason for it. They [the manures] would, in fact, have been equally provocative of the disease in the four series of plots if there had been in them anything inherently harmful, and the first row would not have been the only one to show the effect.

But, even though they do not contribute to the bunt, considered, as I have said, solely as the excreta of animals, neither do they possess any virtue as preventives of the disease. There was as much smut in the non-manured as there was in the manured plots of my experimental tract. The diseased plants, it is true, drew much vigor from the manured portions of the plots where they occurred; but there was always present in them the destructive principle—present from their very beginnings and transmitting the disease into the numerous culms produced by these plants. The first series of beds of the fifth division received no manure. If it, on the whole, produced fewer bunted heads than the first series of the other divisions it was because the growth there was less vigorous; but the number of diseased plants was the same in the contaminated portion of each division. I have noted no characteristic difference in the

disease that one could attribute to the fertilizers, regardless of their nature. In one fertilized plot the disease affected a large number of culms belonging to the same plant. In poorer soil there were only one or two, an individual plant commonly producing no more than that many heads: That was the only difference.

That continuity of plots more or less affected by smut, as recorded in the account of my experiments, has, no doubt, been noticed. It has been observed that the seed sown in them was either blotched or blackened with bunt. It was thenceforth suspected that this dust contained a virus with which sound seed could be infected, and attention was then directed to the seed as the starting point of the disease.

This conception of the matter will be all the more quickly accepted after observing another long series of plots bearing almost no indication of the trouble present in neighboring plots, and that series in which the plots varied one from the other, according to the treatment given the seed.

Would not one's attention have been arrested by the fact that there were as many bunted heads in the plots of the first series as in corresponding plots of each division? Project 1. So that, for example, ⅗ of the heads of the fifth, twenty-ninth, and fifty-third, etc., were diseased and ⅞ of the heads in plots 17, 41, 65, etc. I shall here call to mind what I stated in the beginning of my memoir on the manner in which these plots were sown.

On each date of seeding I worked on each of the 5 divisions and sowed regularly four plots. I used only 4 kinds of seed in each seeding and I sowed these 4 in each of the divisions. By this method, the seed sown in a given bed was the same each day for 5 others. Let us again cite an example: When I sowed the fifth plot I sowed also the twenty-ninth, the fifty-third, etc. It is now easy to see that the seed, being the same for each of these several plots, the end product had to be the same or similar and that the disease, once noticeable in one plot, must necessarily show up in the others in more or less equal amount. Any inequalities in the amount of bunt appearing in one plot as compared with another I attributed to differences in the quantity of inoculum that different kernels may have re-

ceived when I dusted them and to inherent differences in susceptibility to the virus.

It has been possible to note that plots 17, 41, 65, etc., devoted to beardless wheat, were almost wholly infected, and one would be tempted to believe that this kind of wheat is more subject to bunt than bearded wheat. But, among the experiments of 1752-53, I shall report one that seems to prove that this disease attacks bearded wheat with equal frequency and ease, or at least leaves one in doubt about the matter.

I stated that in the second, third, and fourth series of my plots I noticed some smutted heads. The seed sown in those plots, however, was selected and also treated for the second and fourth series. By what possible chance could those plots have developed any diseased plants?

To this I will first reply that in spite of the attention given to the choice of the seed it is almost impossible to escape the inclusion of some bunt balls, contact with which introduces an element of danger to the sound seed.

In the second place I will say that I made a mistake in my seeding in that I always began by sowing the smut-inoculated seed before I sowed the clean seed. It was also always the same hand that distributed the four kinds of seed and which, once soiled by the smut-inoculated seed, could contaminate other seed, even though prophylactically treated, as well as that that had not been so treated. Also I observed that the plots sown to the latter kind of seed bore more smutted heads than those sown to treated seed on which there remained a light residual coating of lime mixed with some particles of nitre and common salt. By the care I exercised in allowing an interval of several days between sowings I was able to do my planting at times when the conditions of the soil and of the atmosphere were the same.

On the 16th of October, date of the second seeding, the weather was mild and the mercury in the barometer stood at 27.9 inches.

The second seeding was done on the 22nd of the same month, following some consecutive rains. On that day there was a cold sharp wind; the barometer indicated an atmospheric pressure of 27.8 inches.

The third sowing date, October 27, was a very beautiful day; the barometer stood at 27.8 inches.

The weather on November 3, the date of the fourth sowing, was quite mild and partly cloudy. The barometer registered 27.3 inches.

On November 10, the date of the fifth sowing, a very cold wind prevailed. A light but nearly constant rain fell. The soil was heavy and sticky and hard to work. The barometric pressure rose to 27.7 inches.

The sixth and last sowing was made on November 22. The day was cold and very sharp; the surface of the ground was frozen; a fairly thick mist hid from view near-by objects; the barometer registered 28 inches.

I neglected to observe the thermometer during these different seeding operations, but one can approximate the temperature variation for the period by the air-temperature readings, which I have in general indicated.

With the exception of the 30 plots of the first series in which the smut developed so abundantly for the reason noted in the foregoing, none of the other plots, although 90 in number, produced any bunt, and that regardless of whether the weather was fair or foul at time of seeding. The plots of the sixth sowing, at which time there was a thick mist—for some farmers, such a bad sign—, were as exempt from the disease as those of the third sowing, done on a clear, calm day.

We shall not dwell longer, therefore, on purely assumed causes of an external character, such as rains and mists that prevail during the growth of the wheat crop; excessive or deficient soil moisture available to the seed and time of seeding. We cannot, I say, longer delay to consider all these things as being capable, in themselves, of causing smut. Even in cases where these conditions are unfavorable, it will always be necessary to assume the presence of a disease germ independent of such conditions and in the development of which they could, at the most, do no more than contribute.

There remains for me to say only a word concerning the moss plot the results of which have already been noted. The first division of this plot, in which I sowed seed blackened with smut, produced bunt in abundance. The three other divisions, in which I had sown selected and smut-free seed, or some of the same seed differently treated, were free from bunt, if one except the second division. But attention al-

ready has been called to the fact that proximity of the first division may have been responsible for this.

Project 1. Let us conclude then, without fear of error: (1) that, since the smut that appeared in the wheat grown in pure moss is just the same as that produced by wheat plants grown in soil, one cannot attribute this disease to the nature of the soil, to a particular kind of substratum.

(2) Since the smut was as abundant in the first division of the moss plot as it was in plots 21, 45, 69, etc. of the first series, it is necessary, in order to determine the cause, to take into account the seed, which was the same for the first division of the moss plot as that used in those particular plots. It is necessary to attribute it to the dust of the bunt-balls with which this seed was inoculated and which was as active in the moss as in the fertilized soil and the soil that had not been fertilized.

Finally, we may conclude that the smut could not have been due to excessive soil moisture, as Mr. Tull thinks. In the two last divisions of the moss plot not a single bunted head was found. Nevertheless, the wheat, in order to complete its different stages of growth, had to be continually irrigated. The moss, from which it derived its substance, was always soaked with water. It threw out numerous roots, which partook of the excessive humidity of the moss and which would have served only to render the disease more certain if excessive humidity had borne any causative relationship to it.

I might again attack in a more direct way the opinion of Mr. Tull. I have performed, I have, in fact, repeated, the experiment reported by M. Duhamel in his treatise on the subject of land culture. I observed but one wheat plant that produced bunted heads. This plant grew in soil allowed to remain continually impoverished. This experiment has been repeated and the details have been presented to the Academy for its judgment as to the weight one should give to the opinion of the English scholar.

I trust that I may be permitted before closing the consideration of my experiments of 1751-52 to make an observation that is, to be sure, somewhat apart from the subject here treated, but that I could not overlook because of its importance. It is based on those same experiments and is intended to convey the fact that a great deal of seed is

lost in the ground, that this loss is due principally to a too deep working of the soil, and that a little seed well distributed affords assurance of greater production than does much seed carelessly sown. In consideration of the light shed on this matter by Duhamel and several other zealous citizens, my reflections will seem, perhaps, out of order and certainly less conclusive than those on which they have depended and to which I feel indebted. My reflections are, however, based on experiments, and one cannot know too much to draw one's inferences.

The arpent of ground, in the country where I am contains 40,000 square feet. It is the practice among the farmers of our cantons to sow 5 bushels or nearly that amount of wheat per arpent. The bushel weighs from 35 to 36 pounds and contains 20 pints, the equivalent of 24 of those of Paris. In an average year the local arpent yield is 30 to 32 bushels. If we put the production at 24 it would be a fair average between the returns of the best harvests, say 40 bushels per arpent and those of the poorest, say 28 bushels.

I stated that the tract where I carried on my experiments was 540 feet long by 24 feet wide. Before doing anything else it is necessary to deduct from this width four feet, which served only as paths between the plots and, therefore, had no part in the production of the area. Five-hundred-and-forty multiplied by 20 gives 10,800 square feet of surface, the equivalent of 27 *cordes*, a *corde* being equal to 20 square feet. At the rate of 5 bushels per arpent an ordinary farmer would have sown on this area of 10,800 square feet one bushel and seven pints of seed; and on the basis of a return of 34 bushels per arpent he would have harvested 9 bushels and 3⅗ pints.

It will no doubt be recalled that my 120 plots, comprising 10,800 sq. ft., were 5 feet wide by 18 feet long and that the wheat was sown in rows, alternately, 5 or 6 rows per plot, according to the day of the seeding. In the first case, the rows were a foot apart and on the borders of the plot there was left a space six inches wide in order to allow to the first and fifth rows opportunity to grow without crowding the alleys. In the second case, where the rows were 6 in number, they were only 10 inches apart and the scant vacant space on each border amounted only to 5 inches.

I used for my 120 plots only 12 pints of seed or thereabouts. This amount of seed would seem excessive to those who carry on their experiments in accordance with the new practice, but I did not control the hand of the sower. The diseases of the crop, more noticeable in a number of plants, constituted my principal interest.

With the advent of spring, I was more than ever impressed with the necessity of a more careful handling of the seed; my plots being covered with verdure, the plants shot up vigorously. I was obliged to pull up part of the plants in those plots where the manure had been too generously spread. I was later provoked with myself for having failed to remove more plants than I did. The remaining plants, vigorous, erect, laden with heads, and more than ordinarily luxuriant had become objects of interest only to be blown down in several directions by a violent wind accompanied by rain. I propped them up with stakes, but in spite of my precautions, the grain suffered from the misfortune: it was shrunken and poorly filled; and, as a result there was a necessary depreciation in ultimate returns. It was, however, better than I had dared to hope, as the details here given will show. In the 30 plots where the smut was so abundant, this question does not enter into this first set of figures.

The first	part of the area fertilized with	pigeon droppings yielded	2	bus.,	16	pints.
" second	"	"	"	sheep manure	"	2	"	19½	"
" third	"	"	"	night soil	"	2	"	16½	"
" fourth	"	"	"	horse and mule manure	"	2	"	11	"
" fifth	"	not fertilized				2	"	14	"
				Total yield		13	"	17	"

The piece of ground treated with horse and mule manure produced a little less than that where no manure was applied, because it contained more lodged plants and it yielded a greater proportion of poor and shrivelled grain.

It is now necessary to add to these 13 bushels and 17 pints the yield of thirty plots that were smut-infested and of which I have, until now, taken no account. I did not thresh the sheaves produced by these invested plots. I kept them intact so as to remove from them the diseased heads and culms in order to provide inoculum for the experiments of 1752-53.

Since it was my responsibility that these 30 plots, constituting one fourth of the nursery, should be as exempt

from disease as the 30 other plots and as productive, the same amount of seed having been sown in those plots as in the healthy ones, their yield should be estimated as being one-third of 13 bushels, 17 pints, the total yield of the 90 healthy plots. I here present my figures:

Actual yield of the 30 healthy plots	13 bus.,	17	pints.
⅓ this yield, the estimated return from the 30 infested plots	4 "	12⅓	"
Total	18 "	9⅓	"

I stated that an average farmer, in sowing a like area, would have employed 27 pints of wheat where I used but 12 and that, too, lavishly. Therein, already, was a saving of 15 pints.

This farmer, I said again, would have harvested but 9 bushels, 3⅗ pints of seed, while I would have obtained or would have been able to obtain 18 bushels, 9⅓ pints. My crop, then, returned to me a little more than double his yield. Thus 10,800 sq. ft., which, in any ordinary year, would have produced at the rate of 34 bushels per arpent, yielded for me, as a result of improved and crudely employed methods, a return of 68 bushels and 7⅘ pints per arpent.

At first glance one might perhaps attribute this marked improvement to the strength and abundance of my fertilizers; but, excepting the fourth division, so heavily fertilized as to be injurious, it has been already noted that the fifth division, where there was no manure, returned a yield of 2 bushels, 14 pints. If one add to this first amount the 18 pints from the 6 plots attacked by smut, this fifth division should have returned a yield of 3 bushels, 12 pints. Assuming a like return from the 4 other divisions, I should always have obtained from the 10,800 sq. ft. yielding 18 bushels, a very much greater return than that we have figured for the ordinary crop of a farmer working an area of the same size.

Continuation of the Experiments and Reflections relative to the same dissertation

The outcome of the different experiments I have presented seems sufficient to persuade me that the disease (bunt) was contagious, and that the virus was resident in the dust of the bunt balls. I scarcely hesitated so to conclude

after looking over that series of 30 infected plots alongside another series of plots where no smut at all appeared, in reflecting on that proportionate number of damaged heads produced by the plots of the first series, which, in each division were found similarly situated: a kind of equality or sameness that was difficult for me to attribute to any other cause than the condition of the more or less smut-contaminated seed; but always the same for the plots where the number of diseased heads was alike.

1752, 1753

However, I was always doubtful as to the purity and intrinsic quality of the seed I had inoculated. I had not forgotten that it was in the main the poorest, the most defective I could find in the different sacks offered for sale, and I always feared that some imperfection peculiar to the seed and apart from the smut with which I had dusted it, was the real cause of the trouble.

My principal aim, then, was to determine whether a healthy, plump seed in perfect condition could produce bunted heads from those only that had been blackened with the dust from smut balls obtained from diseased heads. I felt, too, that I must assure myself as to whether the straw from bunted plants did or did not manifest the disease and if, once incorporated in the manures, it could be transmitted to any seed in proximity to it. I did more, I observed the principal disease of darnel grass, struck as I was by the agreement between it and that of wheat and tried to bring about the latter in the first-named plant by means of spores. I tried some experiments of this kind on rye, barley, oats, etc. and shall now relate all my operations.

They had for an object a goal of ultimate consequence. I, therefore, beg the enlightened company, to which I am under obligations to render an account, to permit me to present the minutest details and that I leave it ignorant of none of the particulars I have observed.

I shall follow exactly the order of my work. After having informed you regarding the preliminary arrangements incident to seeding, I shall say a word concerning the nursery where my experiments were carried on; I shall give their distribution; and shall tell of different kinds of seed and the different conditions, even, of the seed employed. I shall place each kind in the order followed in my sowings so that one will have at the outset a general prospect. I shall relate

in due course and in historical sequence certain observations I made at different stages in the growth of the wheat; I shall give the result of each particular experiment in presenting the reflections these observations provoked in me; and I shall finish by replying to three or four difficulties, which, although too trivial to cast doubt on the facts I shall here disclose, nevertheless merit one's seeking a solution.

The first thing to occupy me at the close of the year 1752 was the composition of the different manures I needed to use. On the 13th of September I had a Spanish horse lodged in a suitable stable. His litter consisted only of straws that had borne bunted heads. I shall refer to it in due course as *suspected straw*. It is known that straw is the ordinary fodder of this kind of horse. In the fear that the fodder fed to the horse, and of which a part fell about his feet, had not become mixed with his bedding, I was careful to feed him for 7 or 8 days, and not without a marked disgust on his part, nothing but bundles laden with considerable bunt. In that way the few bundles that had become mingled with the bedding could hardly have changed the condition of it. A little pile of the litter was drawn from under the horse and was put aside for use.

The stall was cleaned a second time. The same horse had for his bedding some straw the cleanliness of which I was certain; he was fed on more savory bundles than the first ones; also, he ate them with a relish. Within 8 or 10 days, I had the necessary amount of smut-free manure which I put in a pile for use, just as I did the first lot.

During the rest of September I got together the other kinds of manure that I had planned to use. I cut up in small bits the clean straw and mixed it with the ordinary proportion of the manures, as much with the sheep droppings, the horse manure, the sheep manure, the basis of which was hay, as with the cow dung. Each kind of prepared manure was put in a vat and watered several times to hasten rotting of the straw.

Whatever was done to prepare the manures of which the clean straw was a component part was done also in making up those into which the contaminated straw was introduced; the same fertilizer taken from the same heap to mix with the one and with the other; the same attention to the pro-

cedure; the same waterings; no difference anywhere other than that resulting from the straw.

Entertaining, as I did, the idea that the straw of smutted wheat plants might be dangerous for the wheat, I avoided walking on the ground that bore such plants so recently and that still contained the remains of stubble that I mistrusted. I chose a little field hedged in, with a good exposure, by a good seed bed that was to produce rye. Although the tract may have been one of the best in the canton, I spaded it under immediately after the harvest and ploughed it a month later. Some bushels of pigeon droppings were scattered over it, the only kind of non-suspected amendment that it was easy for me to find.

This field was 360 feet long by 74 feet wide. I did not use all of it but limited myself to a piece 360 feet long by 48 wide. The remaining portion was saved for part of the experiments of 1753-1754. I made there 120 plots, 18 feet long by 5 feet wide, laid out in six parallel series and separated by alleys, one foot wide. These 120 plots occupied a total width of only 36 feet by the whole length of the nursery tract. There remained, then, a space of 12 feet running the entire length of the tract. After having been sown, this was worked according to custom.

I followed the same method of seeding I had observed the preceding year. Each of my plots contained 5 rows, the rows were separated by alleys 1 foot wide, and on the 2 edges of each plot there was left a space of 6 inches in excess of the alley for a purpose already explained.

I stated that my 120 plots were laid out in 6 series, each series containing 20 plots, and the plots numbering 6 across the front. The first section for our consideration will be only the 100 plots contained in the first 5 series and designated by their numbers. The remaining 20 plots, which I have designated by letters, will require a thorough cleaning up because they have grown such things as darnel, rye, barley, and wheat, known under the name of mouse wheat [*Triticum cinericeum* G. Bauh.]. It is necessary, for the sake of clarity, that I treat this group of plots separately and consecutively.

Project 2.

Before entering into the details concerning each plot in particular, it is essential to make known the quality of the seed I employed. I withdrew it from several selected bundles

produced by one of the better cantons. I did not fail to remove from these bundles before they were thrashed some bunted heads found in them. The wheat obtained from the bundles was very choice; I noticed, however, 4 or 5 bunt balls that without doubt came out of partly bunted heads. *Triticum cinericeum* G. Bauh. Lib. XVIII.

This selected seed was put into a special bag, and it was always from this one bag that I took as much pure and perfect seed for sowing as I took of that designed to become a source of bunt by becoming infested with this kind of dust. I shall designate the grain taken from this bag by the term *pure*.

I sowed in plot No. 1 some pure seed without fertilizer. This plot and all the others into the consideration of which manure does not specially enter are regarded as having received none. The pigeon droppings, spread on all the plots, exerted no influence on the disease in question and need not occupy us here. Project 2.

The second and third plots were fertilized with noncontaminated straw thrown out from under the Spanish horse.

The fourth and fifth plots were fertilized with contaminated straw from under the same horse. The seed sown in these four plots was pure, and was the same for the other 16 plots that had been manured. In order that the seed might rest immediately on the manure, I had a little furrow made quite deep and the manure put in it. The grain was sown on it and then covered with soil. This operation was the same for the different manures.

Plots 6, 11, and 16, or, for that matter, all plots of the first series, were without special manure and were sown to pure seed only, or to another selected sort, with the exception of the 26th, where I applied some fragmented noncontaminated straw, and of the 76th, where I sowed wheat, contaminated after having washed it.

The 7th plot was again manured like the 2nd and 3rd, and the 8th like the 4th and 5th.

In the 9th plot, I placed manure consisting of sheep droppings and clean straw, and in the 10th, the mixture comprising sheep droppings and contaminated straw.

Plots 12 and 13 contained fertilizer consisting of horse manure and clean straw.

Plots 14 and 15 were manured with a mixture of horse manure and contaminated straw.

The 17th and 18th plots were fertilized with the manure consisting of sheep manure, the basis of which was a litter comprising only hay and clean straw.

The 19th and 20th plots were given the same sort of manure combined with contaminated straw.

The 22nd and 23rd contained manure composed of cow dung and clean straw.

The 24th and 25th, this same kind of dung with contaminated straw.

I have already stated that the 26th plot placed in the first series contained clean, chopped straw. In using this straw, I followed the same method employed in applying the other fertilizers: that is, I distributed it in a furrow dug to a depth of 4 or 5 inches, then sowed the seed on this straw and covered it.

(A) Glumae

I placed some contaminated straw in plots 27 and 28, some clean chaff (A), in 29, and some contaminated chaff in 30.

For plots 32 and 33 I set aside some clean wheat previously moistened with ordinary water in which I had melted some adhesive gum and then dusted it with bunt.

Plots 34 and 35 were sown with clean seed treated as in the two preceding plots but with this difference, namely, that instead of gum, I dissolved some fish glue in the water with which I dampened the seed.

Plots 37, 38, 39, and 40 deserve special consideration. The seed sown in the first two was clean wheat that had previously been heavily blackened with bunt, then moistened in this condition with ordinary water in which had been dissolved some common salt and lime. The seed thus treated lost none of the smut with which it had been dusted. After it was dampened, it seemed as though it had been dipped in very thick ink. The two last plots received clean wheat that had been prepared in nearly the same manner, except that the water with which the seed was moistened contained nitre.

Plots 42, 43, 44, and 45 I sowed with wheat that had been merely blackened with bunt. This seed was placed at different depths. In the first of these plots it was planted in the top soil; in the second at a depth of 1 or 2 inches; in the third, 3 or 4 inches; and in the fourth at a depth of 5 or 6 inches.

Plots 47, 48, 49, and 50 were destined also to receive seed at 4 different depths, which I shall indicate in the same order as has been noted. This seed consisted of noncontaminated wheat, but dusted with ground darnel seed. I have already stated that this plant was subject to the same disease as wheat.

Sequel of the second project.

I had a remnant of the horse manure with which only the contaminated straw had been mixed. I used it in plots 52 and 53 where I sowed clean wheat. I left plot 54 unmanured and sowed in it the same kind of seed employed in plots 52 and 53.

In drawing from my infected bundles the large quantity of necessary smut, I was careful to separate those heads that were partly smutted and partly healthy. These heads, from which I myself removed the sound, germinable grain, furnished me enough seed for one plot. This was plot 55.

In the 57th plot I sowed some clean wheat that I had blackened with smut and had left buried in this dust for more than a month.

I inoculated some clean wheat with dust from some [loose] smutted kernels of wheat, barley, and oats. I sowed these 3 lots of inoculated seed in plots 58, 59, and 60.

I selected from some badly infected bundles several healthy beardless heads and some that were bearded. I removed from the first lot the seed for plots 62 and 63 and from the other the seed sown in plots 64 and 65.

In plot 67, I sowed some clean seed but, before sowing it, I placed in the bottom of the furrow a trail of the dust from some bunt balls, onto which the clean seed fell and remained intact.

The 68th plot was prepared with greater attention to detail. After I had opened with the pick the customary little furrow in which to sow the seed, I placed in the bottom of this furrow a wooden rule 6 lines [½ inch] thick, which occupied the whole length of the furrow, and partitioned it into two parts. On one side of this rule, clean wheat was sown; on the other a strip of smut from bunt balls was laid down. The rule being lifted, the seed was found one-half inch, or nearly that, from the dust. It was then covered with soil, care being exercised not to displace it.

Plot 69 was sown with beardless wheat, blackened with bunt, and plot 70, with similarly inoculated bearded wheat.

Plot 72 contained clean wheat that had been previously treated with an aqueous solution of common salt and lime dust.

Plot 73 was sown with noncontaminated wheat previously pickled in an aqueous solution of lime and nitre; the 74th, with selected mouse wheat; the 75th, wheat of this sort, blackened with bunt.

I stated that plot 76 contained diseased but washed wheat. This seed had been furnished me by the Procurator of a Carthusian nunnery, alongside which institution is situated the field that I worked. The field of the Procurator produced much smut in 1752 and I perceived that certain advantages might result from using this seed under varying conditions. I used it in plots 77 and 78 after having treated it, according to my custom, with nitre or with sea salt and lime. In plots 79 and 80 I sowed it just as it came from the Procurator's granary. In the language of the farmer, it was the last.

Plot 82 contained selected spring wheat; plot 83 some of the same wheat dusted with bunt; and plot 84, some wheat similar to that I had at first moistened with an aqueous solution of mucilage and later dusted with bunt.

In plot 85 I sowed one seed at a time, 6 inches apart. I used miracle wheat, known to botanists as *Triticum spicâ multiplici*. This plot contained 5 rows, 3 of which comprised clean, nontreated seed; the remaining 2 rows contained seed with which had been mixed some seed of the same kind that I had dusted with smut.

The 12 plots, presently to be considered, were not sown until in March, 1753, but immediately following the others. Let us not interrupt the thread of our discourse.

I sowed some clean common barley in plot 87; some barley blackened with barley smut in plot 88; in plot 89, some barley dusted with wheat bunt; and in plot 90 some barley seed treated with nitre and lime after having been blackened with barley smut.

Plots 92, 93, 94, and 95 were set apart for spring wheat. Clean seed was sown in the first plot; clean seed also, with contaminated straw in the second; blackened with bunt spores in the third; similarly inoculated in the fourth but, before sowing, treated with nitre and lime.

Oats occupied plots 97, 98, 99, and 100. The seed was

sown under the same conditions as to treatment or absence of treatment as was that of the barley.

I now take up the 20 plots of the sixth series: In plot A, I sowed some healthy darnel seed free from all suspicion as to its purity. In plot B, I sowed some sound darnel seed that, prior to sowing, had first been treated with nitre and lime. Second Project

In plot C was sown disease-free darnel seed after having distributed in the furrows a large quantity of spores from the bunt balls of infected wheat. Plot D was devoted to healthy darnel seed dusted with bunt spores. Plot E, to healthy darnel inoculated with darnel smut; and plot F with healthy darnel inoculated with darnel smut, then washed.

In plot G was sown some darnel seed collected in a field of rye where there were found many other darnel plants attacked by smut.

The 4 plots marked H, I, K, and L were devoted to rye. Clean, nontreated seed was sown in plots H and I, seed inoculated with wheat-bunt spores was sown in plot K, and seed inoculated with the dust of some ergot grains I had reduced by grinding was sown in plot L.

Plot I differed from the others in that its seed furrows had been dusted with wheat bunt right where the rye kernels fell.

The 7 plots designated M, N, O, P, Q, R, and S were devoted to square barley (*Hordeum polistichum*). It was sown as clean seed in plots 1 and 2; (the five seed-rows of this second plot had been inoculated with wheat-bunt spores) inoculated with wheat bunt in the third and fourth; blackened with barley smut in the fifth and sixth; and the seventh plot was sown to wheat-bunt-inoculated seed, subsequently treated with nitre and lime. Continuation of the second project

I sowed mouse wheat in the two last plots. For the one marked T, the seed was dampened with mucilage water and dusted with bunt. The other, marked V, was sown with clean, nontreated seed.

It may be recalled that alongside the sixth series of plots I had reserved a space 12 feet wide and 360 feet long, the entire length of the area, and that I had divided this strip of ground crosswise into 3 equal parts. I sowed clean wheat in the first; some of the same kind of wheat in the

second but heavily inoculated with bunt and then allowed to stand a month before restoring it to its original condition by washing in several waters; and in the third plot I sowed clean wheat, which was first inoculated with bunt, allowed to stand for a month in this condition, and then washed superficially and again covered with a new coating of bunt to the extent of forming a sort of crust on the seed.

In this little tract of ground, of value to me because of what developed there, the seed was covered with the plow, according to custom. Before this was done, however, I took care to mark with a stake the furrow that separated each part and that had served to govern the seeding operations. I next had some barley (*Hordeum polistichum*) sown exactly opposite the stakes and thereby always had before me a visible object with which to mark off distinctly the 3 parts of the tract.

The rachitic condition is always the first disease to be observed in wheat. It shows in a very marked manner after the beginning of May. I had every opportunity in 1753 to make some observations on this peculiar condition, and I became more and more inclined to the opinion that insects contributed to it. The aborted or rachitic wheat plants are much more prevalent than one imagines. In the great number I have examined there were very few in which I did not discover staphylinids of every age, even on heads tightly enclosed within the boot and not more than half an inch long.

Three things principally have impressed me in observing rachitic wheat plants. I have noticed: 1. That they were ordinarily more advanced than the healthy plants, other things being equal, and that they produced heads sooner.

2. That the stem and the still-enclosed leaves of these diseased plants, on which are found some insects, are studded with some droplets of a very clear liquid, which seems to be exuded sap.

3. That their first leaves are ordinarily either withered or decomposed; that the part of the stem that is in the ground is often altered and shows a little of the decay shown by the first leaves.

Will the naturalists, accustomed to follow insects, find nothing in the phenomena of which I speak that resembles

those reactions often occasioned by these same insects in other plants?

The examination of the blighted plants occupied only a few days; I passed soon to a consideration of the bunted plants. I was so accustomed to distinguishing them from the bunt-free plants by their external characters that I very early recognized the disease in the special plots and in the third part of the plot worked by the plow, where the seed had been dusted and where I had already anticipated bunt would appear.

The great abundance of diseased plants I had at my disposal afforded me every facility to apprehend the malady in its beginnings and to follow its progress. I, nevertheless, renewed the observations I had made the preceding year on the bunted wheat. General stunting of the plant growth, high mortality among the plants, bluish color of stem and leaves, first hint of green in the youngest grains, their fetid odor when crushed, all agreed with what I have already said and would here constitute a needless repetition were I to describe them. I shall pass to the consideration of the different results obtained from my plots and from the little strip of ground alongside them.

I first collected my results while the plants were still *in situ*, waiting until after harvest to count scrupulously the healthy as well as the bunted heads produced by the plots. It will here be a matter of a kind of summary of the first estimate and of the exact calculation made after the harvest. It will be easy to judge of the particular product of each plot by glancing at the general plan of my operations and at the results recorded for each.

All of the first series of plots was excellent except for a few evident heads of bunt. Each plot, one with the other, bore 5 or 6 such heads. There were more of them in the seventy-sixth plot, as it had received a different sowing from that of the others and was under suspicion.

In general, there were more bunted heads, although in some instances but slightly more, in the plots prepared with manures containing contaminated straw than in those where the manures contained clean straw. The difference was especially noticeable in plots 17, 18, 19, and 20, where I had used two kinds of sheep manure. Plot 18, more affected

Second Project.

than 17, contained only 94 bunted heads in a total of 3,569,[1] while plot 19 contained 825 heads of bunt in a total of 3,325, and plot 20, 879 bunted for the total product of the plot, which was 3,257 heads. These two last plots had received the same seed as the first two but, at the same time, some contaminated straw had been placed in the seed row. One should hardly attribute this abundance of smut merely to the contaminated straw in these manures.

In considering the manured plots and especially those containing cow dung, I at first thought that the straw, of whatever sort, offered no element of danger to the seed; but plots 19 and 20 made me apprehensive, and it soon will be seen that I had cause for suspicion, for, when such straw contained some diseased heads, it contained a virus that could not be too much feared.

It would be marvelous if the contaminated straw, once buried in the manures, lost all its banefulness.

According to every indication it did not lose its harmfulness in the mixture I made of it with sheep manure. But I should note here that this manure was very dry when I used it, that it was easily reduced to a dust, that it contained much hay and, therefore, may have lacked sufficient action and heat to reduce to a certain state of decay the contaminated straw with which I had mixed it. This year I performed again the experiment involving use of the different manures. That part of the experiment relating especially to sheep manure mixed with contaminated straw will command my attention.

The wheat of plots 27, 28, 29, and 30 failed to resist the rigors of winter. Its roots, interlaced in the straw and not directly covered by the soil, were frozen nearly their whole length. This loss I felt. I also experienced a little damage in two other plots of the same kind, to be considered in their turn.

Plots 32, 33, 34, and 35, purposely sown to bear smut, yielded a sufficiently large quantity of it but less than I had

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[1] One may regard this account, as well as those accounts that will follow, as done with all possible care. I have strongly felt that one might hesitate to accept the results I would have to give for several plots if I produced only simple estimates. I am, therefore, led to the point of culling from a large number of bundles healthy and diseased plants, healthy heads along with diseased ones, counting the one and the other and of giving the same attention to the plants bearing healthy and diseased heads as to heads bearing both healthy and diseased kernels.

predicted. Perhaps the mucilage and fish glue that entered into the treatment of the seed formed on its pericarp a light glazing, which arrested in part the action of the dust with which the seed had been inoculated.

The virtue of sea salt and saltpeter, especially in combination with pulverized lime, in preventing the effect of the bunt dust on the healthy seed is clearly shown by the condition of plots 37, 38, 39, and 40. They were very luxuriant; I could find scarcely no diseased heads in them. They were in the midst of the contagion. The 4 plots that preceded them and those that followed bore an abundance of smutty heads. Moreover, even seed taken from the same sack for these 12 plots and inoculated with the same spore dust in equal dosage was, in part, unaffected by the inoculum. What is the conclusion? That the remedy appeared to be effective.

In sowing some smut-inoculated seed at different depths in plots 42, 43, 44, and 45, my purpose was to examine (1) whether the wheat, sown at a depth of 6 inches and before it had thrown out several roots prior to emerging above ground, could, by virtue of its many roots, acquire a vigor that would render it less liable to disease attack and diminish the effect of it. But the virus, once incorporated in the germ of the wheat seed, is too active to be arrested by any strong vegetative growth. In plot 45, where the seed had been sown 6 inches deep, I had only 70 healthy wheat plants, bearing 331 heads; while, in the same plot, I found 211 diseased plants, bearing 938 heads of bunt. Several plants, however, were very vigorous; one, among others, producing as many as 27 culms bearing heads measuring 7 to 8 inches in length. Three-fourths of the plants in this plot were, therefore, attacked by the disease and, although they were strong plants, they were not proof against the virus. I should, nevertheless, state by way of qualification of what I have said that a large number of these sick plants bore heads some of which were healthy and some were bunted.

The second objective I had in mind in sowing the seed at different depths was, in fact, to prove that wheat, although grown from shallow-sown seed, may survive with vigor and in excellent condition, the rigors of a very cold winter, and that it perishes in great part if the seed be

planted too deep. This same plot 45, of which the seed rows were 5 to 6 inches deep, bore only 281 plants and 1,269 heads, although it had received the same quantity of seed as was sown in plot 42, where the seed was sown in the topmost layer of soil and the yield was 1,057 plants, bearing 3,713 heads.

The seed of plots 47, 48, 49, and 50 had been inoculated with dust from bunted seeds of darnel. This dust was almost as disastrous to wheat as was that of the wheat-bunt balls to the 4 preceding plots. This fact was astonishing; but 4 plots, where the phenomenon was equally noticeable, seemed to enable me to make certain of it. It will appear very singular, moreover, that this darnel bunt, which is quite similar in its color to its Spanish relative, left no hint of it in the seeds attacked by it. Its seeds, in fact, though ruined solely by the virus that this dust conveys to the seed, which in turn produces it, inevitably become changed to a black dust and differ in no respect from those that become bunted in any other way.

The conclusions I have drawn in speaking of the 4 previous plots must bear some relation to these latter ones. Vigor of plant growth is incapable of arresting the action of the virus. A great quantity of the seed perished solely because it was planted too deep. We have seen in the account of plot 45 that nearly ¾ of the seed planted there perished. The loss was not so great in plot 50, however, where the seed furrows were 5 or 6 inches deep, as it was in plot 45. Plot 50 bore 662 plants and 1,901 heads, 998 of which were bunted; while plot 48, which was of the same order, comparable with plot 50 and in which the seed had been sown at a depth of 2 inches, bore 1,143 plants and 3,092 heads 898 of which were bunted.

Nearly ¾ of the seed lost in plot 45, because of the excessive depth of the furrow, nearly half smothered also in plot 50 for the same reason, left no doubt that a considerable quantity of seed was lost in resistant or too firm soil and that the recommendation of the new culture is one of those most widely conceived.

Continuation of the second project.

The results of plots 52 and 53, where I had applied horse manure mixed with contaminated straw, were quite the same as those for the other plots where this kind of manure had been employed; that is to say, the bunt heads were few

in number but fewer still, however, than in the plot where only clean wheat had been sown as, for example, in plot 54.

The healthy grains of wheat, which had been picked out of a partly smutted, partly sound head, apparently contained no dangerous leaven; at least, it is true that plot 55, where I had sown only this kind of seed rescued from contagion, bore only bunted heads.

It would seem from the condition of plot 57 that the wheat that had remained covered for some time by the smut dust from the bunt balls, before being put in the ground, acquired no more disposition to produce smut than when sown immediately after being inoculated. In this plot only ⅖, or thereabouts, of the heads were affected. The damage is sometimes greater, although one may have taken less pains to occasion it.

I should say, to be sure, that this plot was poor in stand and quality, although the seed sown there had not been more sparingly used than in the other plots. Perhaps the cause, whatever one may suppose it to have been, for the bad condition of this plot and of the loss that obtained for a considerable part of the seed, resided chiefly in the kernels that were most infested with bunt and had only a slight effect on those into which the virus had penetrated less deeply.

I was much surprised in examining plots 58, 59, and 60 to find in them, especially in plot 58, more heads of bunt than I would have believed present. The seed sown in the 3 plots was from clean wheat that I had dusted with the spores from smutted heads of either wheat, barley, or oats. The loose smut, the dust, which is a result of it, has it also some contagious malignity? I am not yet able to persuade myself. Loose smut is a disease sometimes combined with bunt. Some bunt balls were found in heads of loose smut. These I used in inoculating the seed of plot 58. It would then have been less astonishing had I found in it a larger quantity of diseased heads than I anticipated.

Plots 62, 63, 64, and 65 contained but few bunted heads; enough, however, to cause me to get the workmen to abstrain from using the kind of seed I used. These bunted heads had been culled from several heads, as many bearded as beardless, drawn out of bundles containing an abundance of bunt.

The dust from the bunt balls that I had spread in the bottom of the seed furrow of plot 67 was completely effective. The seed that lay upon it soon contracted the poison and gave rise to a large number of bunted heads.

The disease was not less manifest in plot 68, though the seed, carefully placed in the furrows, was put a little way from the train of spores I had sown there and none of the kernels could touch it except through the roots they might push out. The seed I sowed in this plot was that same clean wheat, which, used alone in 30 places, gave, so to speak, no smut. We must, therefore, conclude that, if it occurred in this plot, it was because even the roots, yet hardly in their infancy, were susceptible to the virus contained in the smut, for, in this case, it was only through their intervention that the seed could receive and transmit it.

Plot 69 bore 3,117 beardless heads of which number 1,476 were bunted. Plot 70 produced 3,329 bearded heads, 1,705 of which were affected by bunt. The seed used in these two plots had been equally heavily inoculated. It would seem that one would be justified in concluding, against the opinion of some writers, that bearded wheat is more subject to bunt than is beardless wheat, but a single experiment does not warrant a decision; it simply serves to cast some doubt on any advantage that might follow the sowing of the one in preference to the other.

Although the seed employed in plots 72 and 73 was in no wise under suspicion, I treated it with a combination of lime and saltpeter, or lime and sea salt. I am happy to state that I observed at the time not a single head of bunt in those 2 plots. Four or 5 were, however, subsequently found there, and I could not see at all that the remedy had been of any advantage. From the same seed, used several times without treatment there developed, also, a few heads of bunt.

The mouse wheat, which occupied plot 74, gave me the satisfaction not afforded by the 2 preceding ones. I observed in it no heads of bunt; even in plot 75 there was no great number of them where the same seed of this variety had been dusted with smut prior to being sown. This kind of wheat seemed to me less subject to bunt than the others; but, on the other hand, the rachitic condition was quite common in this variety.

The contaminated wheat of the Carthusians, sown in plots 76, 77, 78, 79, and 80, responded, so far as concerned healthy or diseased heads, wholly as I had expected. Plots 79 and 80, where I had sown it in its imperfect condition, bore a large number of bunted heads. Plot 76, where the seed had been washed before sowing, contained much less smut than the other two. Plots 77 and 78 were excellent; bunted heads were very rare. The seed of these two plots also had been treated with lime and saltpeter or with a combination of lime and sea salt. As one may easily observe, from the little bit of smut sometimes left on the brush end of the seed, sometimes nearly all removed by lotions, sometimes inhibited by the action of seed treatment, there resulted some marked differences in the 5 plots in which the wheat of the Carthusians had been sown.

Spring wheat is very subject to smut. I therefore sowed some winter wheat in plots 82, 83, and 84, my idea being that the result might prove better with this kind of wheat. My conjectures were poorly founded. Plot 82, it is true, produced only a very few diseased heads, but the seed sown in this plot was clean. The 2 other plots, the seed of which had been inoculated before being sown, produced smut in abundance: half of the heads were ruined. These 3 plots gave a thin stand; I presume, therefore, that spring wheat is not so resistant to severe freezes as is winter wheat.

The Miracle wheat, sown in plot 85, produced no bunt heads, although I had inoculated much of the seed sown there. I observed not the slightest trace of the disease in another plot where I had sown the same kind of wheat, taking the precaution to inoculate some of the seed. Nevertheless, I would not yet dare feel too confident that Miracle wheat is not subject to bunt.

I am better informed as to common barley. It seems to me certain that it is not at all subject to this disease, even when the seed is smutted with the bunt of wheat; but, on the other hand, it is very subject to smut. There were, however, few heads of smut in the 4 plots where I had sown the barley: they were limited to 6 in plots 87 and 89; to 39 in plot 88 and 35 in plot 90. Would, therefore, the dust of the smutted kernels constitute an element of danger for the seed inoculated with it? Would saltpeter and lime have any effect on it? Plots 88 and 90 would lead one to suspect it.

Plots 92, 93, 94, and 95 were devoted to spring wheat. Clean seed was sown in the first; consequently, it produced only 8 diseased plants, bearing 15 bunted heads, while in the same plot there were 1,484 healthy plants, bearing 3,124 sound heads. Such was not the case in the second of these plots. The same spring wheat, out of the same sack (and this is worthy of note) had been sown in this plot, clean and copiously, from fear that part of it might die, on the chopped winter-wheat straw and absolutely similar to that used as contaminated straw in the composition of the different manures. This spring wheat, clean as it was, produced 676 diseased plants bearing 1,165 bunted heads and 2,025 healthy plants, bearing 3,215 sound heads. Thus the disease was present in more than one-fourth of the total number of heads. Could one attribute to anything other than to the contaminated straw this large amount of smut? It was still more abundant in plot 94 and that, because the seed, prior to sowing, had been inoculated with bunt. It was not, on the contrary, manifest in plot 95, except in very small quantity, because the seed, after it had been inoculated as in plot 94, was treated with saltpeter and lime. In addition to the fact that plot 95 was remarkable for the small number of bunted heads, it was also, by virtue of the vigor of the plants and length of culms, taller by 7 to 8 inches than the stand in plot 94. The treatment produced a double effect: it protected the wheat against the contagious disease and favored its complete growth.

The condition of the four plots that we shall consider forces one to reflect on the danger resident in the use of contaminated straw and on the existence of a contagious poison in the dust from the bunt balls and on the success of the remedy employed to prevent the effect of the poison. Spring wheat seemed to me to be more subject to loose smut than did winter wheat.

Bunt attacks oats no more than it does barley. This plant shows no symptom of the disease through any method yet employed to communicate it. Plots 97, 98, 99, and 100, sown to oats, were very luxuriant, a good stand and free from trouble. I observed in each only 4 or 5 heads of smut. The spores of smutty oats with which I had blackened the seed of plots 98 and 100 produced no effect.

I was able to save but few plants in the majority of the

remaining plots. They were sown on the 7th of November, 1752, at a time when the soil was extremely dry. The darnel grass, the rye, and the barley in those plots remained in the soil a long time before germinating, winter overtaking them in an incipient stage. The frosts were severe; nearly all the plants died. This little setback, however, but served to instruct me in the necessity of sowing rye and barley early to avoid its being injured by the cold while it is yet delicate and meagerly supplied with roots.

In plot A, where I had sown healthy darnel seed, I had no bunted heads among 141 plants I had collected. Of the 240 heads of darnel obtained from plot B, I found none affected with bunt. The seed sown in this plot had been inoculated with spores from smutted heads of darnel and then treated with saltpeter and lime.

Moreover, I did not observe any smutted heads of darnel in plot C in which the seed-furrow bottoms had been dusted with bunt of wheat prior to sowing the seed in them. This plot contained only 135 plants.

The darnel seed sown in plot D had been inoculated with spores from the bunt balls of smutted wheat. In spite of this, however, of the 230 heads produced by this plot not a single diseased head was found among them.

The scene now changes. The seed of plot E had been dusted with the spores of smutted darnel grains. Of the 320 plants produced by this plot, there were 224 healthy ones and 96 stricken with smut.

The seed of plot F was first inoculated like that of plot E, but washed some time after and sown in that condition. Of the 346 plants found left in this plot, there were 217 sound ones and 129 the heads of which were smutted.

I had sown in plot G healthy darnel seed taken from a bundle in which there was a large number of smutted heads. Nothing untoward resulted from this so far as concerned the smut; for, among the 464 plants obtained from this plot, not a single smutted head was found.

From what is to be observed in the product of these 7 plots we conclude (1) and certainly, as will be seen from the account of another plot, that the smut peculiar to darnel is contagious for darnel just as we already have noticed it to be pathogenic for wheat.

(2) That when the darnel seeds have once been inocu-

lated with smut from the same kind of grass the solutions may remove the larger portions of this contagious dust but not eliminate a certain malignity that it has conveyed to the seed.

(3) That there is some indication (I am yet sure of nothing) that the bunt of wheat is innocuous on darnel.

(4) And, finally, that the remedy of which I made use is one of the best, since plot B, where it was applied to the smut-infested seeds of darnel, contained not a single diseased head in the 240 heads it produced.

In the few plants I obtained from plots H, I, K, and L, I observed that the bunt of wheat had no effect on rye; also, that this disease never attacked it. The one with which rye is commonly stricken is ergot. I observed several ergotized heads among those produced by plots I, K, and L, but not enough of them in plot L to lead one to suspect that the dust from ergotized grains was endowed with anything of a contagious nature.

I believe it is now more firmly than ever established that ergot is the result of the sting or puncture of an insect. In examining a large quantity of ergot grains from rye I noticed that several contained a worm, hardly visible to the unaided eye, which, without doubt, had originated and fed there. I enclosed 20 such worm-infested ergot grains in a glass goblet covered with parchment. There the little worms lived, grew, and nearly consumed the ergots. If I can follow them through their metamorphosis, I shall be able to recognize from their appearance the insect that has produced them and that should be the same as the one that deposited the eggs in the ergot within which they were hatched.

Four-row barley certainly is not subject to bunt. I have every reason to believe it is not more so to loose smut. Among a considerable quantity of heads obtained from the 7 plots marked M, N, O, P, Q, R, and S, I found none in which I could observe the slightest vestige of one or the other disease.

I have already stated that mouse wheat is less subject to bunt than ordinary wheat. The results obtained from plots T and V confirm this assertion. In the first was found only a moderate quantity of bunted heads, although the seed sown in them had been abundantly dusted with smut. In the second plot, sown to clean seed, it was with difficulty

that I found one or two diseased heads. The stunted condition was noticeable here, as in all the other plots devoted to the same kind of wheat.

The little tract of plowed ground the laying out of which has already been noted, alone gave me, on the point I sought to establish, such a degree of assurance as would have sufficed in a less interesting investigation. The first part of this plot was notable for the number and height of the stems, for the excellence and healthy condition of the heads. It was only after a long search that I was able to find some bunted heads.

The second part, fairly good but less uniform than the first, contained a large number of bunted heads.

As for the third, it was in a lamentable condition, but essentially satisfactory to me. A thin poor stand in several places, it bore only weak, stunted, bluish plants, infected throughout with a prodigious amount of bunt, it yielded up a very disagreeable odor and led me to suspect thenceforth that it might be baneful to the nascent kernel stricken with it.

The seed in these three different parts of the nursery was precisely the same and came out of the same sack. The healthy, vigorous condition of the first plot indicates, therefore, the advantage of using none but healthy, well-selected seed, even without any previous treatment. The unsatisfactory condition of the second part warns one of the risk run by sowing contaminated grain, even after washing it several times. The diseased condition of almost all of the third plot, therefore, proves convincingly that the dust from the bunt balls of wheat contains a virus, a hidden malignity, that passes into the most healthy plant and intimately permeates it.

Project 1.

The tract, 540 feet long by 24 feet wide, where the experiments of 1751 had been carried on, served me for two others in 1752.

The first series of plots, almost entirely infected with bunt, furnished me a favorable occasion to make a demonstration the object of which was of some practical value. In this first series, the fifth plot of each division was one of those where the disease was most manifest. There I pulled up by the roots the stubble remaining after the plants were cut. This same operation obtained in the 6 plots of the

division. Immediately after, the entire series was worked and the stubble was buried in the 20 plots where I had left it. This series of 30 plots, which formed no more than a neck of land, was sown to selected but nontreated seed.

It is well known that my object here was to observe whether, in a field that had produced much smut, a large quantity of stubble left there unrotted would constitute a source of danger to seed sown in such a field immediately after harvest. Fortunately, it turned out, by an exact comparison of places where the stubble had been taken away with those where it had been allowed to remain, that in spite of every suspicion at first entertained, the stubble was not an element of danger to the seed. I had very little smut in the entire series; and the 4 first divisions contained as little of it as did the fifth. In the general conclusion will be seen the deduction I have drawn from this experiment by combining it with others of the same kind.

The remainder of the tract, comprising 3 other series, was devoted to spring wheat. I conformed to the customary practice as to date and manner of sowing with one difference, namely, this: From the lot of seed designated for sowing this tract I kept out enough to sow an area 40 ft. x 18 ft. This area, as is easily seen, was taken from the same tract of which I am speaking. I inoculated some winter wheat to sow in this small area. The entire tract was seeded the same day and both kinds of seed were covered to the same depth with the plow.

From the month of April one could distinguish from a distance the little piece where the inoculated seed had been sown. The heads of wheat were somewhat poorer than those elsewhere in the tract; they were of a yellowish cast, and I did not know why they did not thrive. In due course they assumed that bluish color characteristic of bunted heads and matured by bearing almost nothing but heads attacked by smut. The rest of the tract, on the contrary, showed but few indications of the disease; I could scarcely find a single bunted head. This piece was crossed by a footpath and the little piece of smutty wheat was very close to this path. Many people had occasion to consider it; and they were surprised to see the disease thus concentrated within a limited area.

The tract as a whole produced a large number of heads

of loose smut: the spring wheat, it seemed to me, was more subject to it, but I hesitate to assert that it was also more subject to bunt than were the several varieties of winter wheat. M. Duhamel is not of the same opinion; and that was enough to make me hesitate. However, it must be said that the particular variety of spring wheat that I employed, consistently produced a prodigious quantity of smut [bunt] every time I wished to make it do so.

Continuation of Experiments, p. 114.

In reviewing the work of M. Duhamel, which I cite here, I am of the opinion that he did not agree perfectly with certain facts I have already disclosed; and I felt that it would be useful to narrate here, for the sake of clarity just what this scholarly academician has overlooked.

"There are," said M. Duhamel, "many heads the kernels of which were filled with a black dust instead of white flour. It is a disease common to several kinds of grain; it is called blast or *smut*, and the grains attacked by it are said to be blasted or smutted."

"Wheat, spring wheat, winter barley, barley, oats and, I believe, darnel, as well as several kinds of couch grass, are subject to these diseases, but certain grains more generally than others. Spring wheat, for example, is less susceptible than winter wheat."

"I have never seen smutted rye."

M. Duhamel designates by the term *blast* the disease I have characterized by the term *smut* [*loose smut*] and applies the name *smut* to the disease I designate as *bunt*. I shall, for the moment, employ the expressions used by M. Duhamel, for it will serve to clarify a part of his publication where they are used.

Wheat, both winter and spring, is unquestionably subject to loose smut and bunt.

I have every reason to doubt that 4-row winter barley is subject to loose smut of wheat and I am certain that it never becomes afflicted with bunt.

Barley and oats are subject only to loose smut.

I have not seen darnel grass affected by loose smut but this plant is very subject to bunt. Up to now, or I greatly deceive myself, this latter disease had not been recognized in darnel grass.

Rye is subject neither to loose smut not bunt.

In general, the first of these diseases is much more com-

mon than the second. In addition to the plants under consideration, among which it is very common, I have noticed it on 4 (a) others the first 2 of which are perennials and the other 2 annuals.

(a) *Gramen avenaceum*, elatius, jubâ longâ splendente. Raji synopsis. 260

Bromus panicula patente, spiculis ovatis. *Linn. flor.* Succ. 84.

Gramen arundinaceum, acerosâ glumâ, nostras. Park Theat. 1723.

Milium vulgare.

In the main, the experiments I have described present something very striking. They will certainly carry conviction when I shall have compared them with other experiments conducted on a small scale and over which I had every opportunity to watch, and with which they agreed perfectly.

The same garden surrounded by buildings, concerning which I have already spoken in referring to some tests I made in 1751, served my needs for the operations of 1752. This garden, somewhat irregular in shape, consists of two squares.

In the first square I laid out 2 plots, each 28 feet long by 5½ feet wide. I sowed in each plot 8 rows of wheat, one seed at a time, and 3 inches apart in the row. The first
Project 3. 4 rows contained clean seed only and in the 4 remaining ones were sown only wheat that had been inoculated with smut.

The first 2 rows of plot B were of clean, smut-free wheat. The third and fourth rows contained similarly clean wheat, but moistened with dilute mucilage and then dusted with bunt. The fifth row contained seed that I had picked from heads of wheat that were partly smutted and partly disease-free. The sixth was sown to some contaminated seed from the monastery, which I had washed. The seventh and eighth rows contained some of this same contaminated seed, but unwashed.

Project 4 I shall not enter into the details of the product of these 2 plots. You will be able to get these at a glance from the 2 plot diagrams at the close of the text.

The second square of the garden contained 5 plots, 15 feet long by 3 feet wide. In each of the plots marked C, D, E, and F there were 4 rows. The first 2 of plot C contained

clean seed and the last 2, the same kind of seed but inoculated with smut.

The first 2 rows of plot D were sown to clean seed that I had first smutted and then washed. The remaining 2 rows contained seed of the same kind, inoculated after having been moistened.

The clean wheat, which comprised the 4 rows of plot E, had been sown as follows: 2 rows on a mixture of horse manure and clean straw; and the remaining 2 on manure of the same kind mixed with contaminated straw.

Some disease-free darnel seed was sown in the first 2 rows of plot F and some seed inoculated with bunt dust peculiar to the species was sown in the remaining two.

The location of plot G did not permit me to make any rows lengthwise, as in the other plots; There were virtually 13 of them crosswise of the plot. The first 6 were sown to clean seed on clean straw; the 7th to darnel seed inoculated with the dust of darnel smut; and the last 6 to clean wheat on contaminated straw.

Again, one will easily perceive at a glance the result of these 5 plots alongside the plot divided into 2 squares. I come now to some observations that, as well as 2 others, I have had opportunity to make.

In spacing the seeds in the row one does not know which plant will be endowed with the greatest vigor and throw out the greatest number of tillers. This vigor, as we have already remarked, is incapable of arresting the action of the virus, once it becomes incorporated within the plant. I present here new evidence in support of this fact.

Project 3.

In a total of 445 wheat plants produced in the 4 rows of plot A, 333 of them were attacked by bunt. The vegetative growth in these 4 rows was, however, quite rank, considering the location of the plot, and the scant amount of air available to it. Each plant produced 4 to 5 culms.

In washing the contaminated seed before sowing, a real benefit obtains; but this salutary treatment is not sufficient to protect the resulting plants from the disease. The seventh and eighth rows of plot B, sown to contaminated wheat, produced in one row 32 diseased and 72 healthy plants and in the other, 49 diseased and 67 healthy plants. The sixth row, in which was sown some of the same contaminated seed that, before sowing, had been washed, pro-

duced only 7 diseased plants in a total of 108. There was certainly a marked advantage obtained solely through the lotions; yet, 1/15 of the crop was lost.

Project 4. In plot C, 2 rows of clean wheat were separated from the 2 inoculated rows by a space of only 7 or 8 inches. This proximity of the 2 sets of rows afforded me the advantage of following the progress of the healthy and diseased plants and to compare their respective conditions. When the plants had attained a height of 4 or 5 inches I began to notice a certain abnormal aspect in the plants of the 2 last rows. The leaves of the ailing plants in due course took on a bluish cast, but remained always narrower by 3 or 4 lines than those of the healthy wheat. The inequality in height of the culms in the 2 sets of rows was maintained throughout; it seemed as though those of the last 2 rows had been cut so as to permit the healthy rows to dominate in height by as much as 8 inches.

So large a number of bunted heads as 933 out of 1,401, the total of the first 2 rows of plot D, is still further proof that otherwise very sound seed wheat that has been allowed to stand a long time in a mass of bunt dust contracts a poison that in great measure prevails even after the seed has been washed in ordinary water.

It is seen from the results of plot E that the suspected straw was harmful to the seed. The 2 rows where this straw was incorporated in the manure, contained 435 bunted heads, while the other 2, where the manure had a basis of clean straw, contained only 244.

Probably nobody will longer doubt the malign effect produced on darnel by the smut peculiar to it when he sees the result in plot F. The darnel seed I employed in these 4 rows came from the same little sack. On the one side, it produced scarcely any but sound, healthy plants; on the other, it caused a quite general disaster.

I have already stated that the bunt of darnel has all the external qualities peculiar to the same disease in wheat. Insidious destruction of the stamens; yellowish color of the tips of the ovaries; floral failure; extraordinary enlargement of the nascent fruits; their greenish tint when they begin to break down; fetid odor when crushed—all similar to what obtains in wheat, except for this difference, namely, that in darnel the farinaceous substance of the seed, which should

become smutty, takes on an ashy white color and finishes by changing into a dust quite like that of Spanish bran. The floury substance of the bunted wheat grain, on the other hand, assumes a blackish tint on the slightest approach of the disease, and retains this color while changing to a dust. I have not, at all, perceived, in examining those 4 rows of darnel several times, that there was, as in the case of plot C, any difference between the first pair and the last, either in the height of the stems or in the color and width of the leaves. It would be very astonishing if, in a darnel plant, the seeds alone suffered from a virus that reached them only by way of the canal of the plant, without the plant itself bearing some external signs of its effect.

The result given by plot G merits some attention. The first 6 rows produced only 11 bunted heads out of a total yield of 783, while the last 6 produced 122 out of 942, their entire yield.

The result obtained from plot 93 has led us to suspect with sufficient reason that the contaminated straw contained something harmful to the seed. Have we not had opportunity here to substantiate this deduction? Is it not striking that there was not a single one of those 6 last rows that did not bear more bunt heads than all of the 6 first ones, together?

Continuation of the Project 2.

By the beginning of September, 1752, I had need of a considerable quantity of contaminated straw for making up the fertilizers. After I had gleaned the stems, laden with their infected heads, from a large number of infected sheaves, I tied them into little bundles and threshed them in a barrel in order to get out the bunt balls. I took pains to remove all of them from the heads. In order that none of the dust remain attached, these heads were beaten with a beetle and rubbed vigorously with the hands.

Those who have seen bundles threshed in which bunt is abundant and have seen the grain from them winnowed, know how much the dust from the bunt balls, broken by the flails, spreads in the barn and what quantities of it become attached both to the wood and to the straw.

The barrel where my bundles of smut were threshed, was by chance very near the second square, next to plot F, quite far from plot G, sheltered, moreover, by a building and some distance from the first square.

It is not astonishing that the abundant dust that came out of the barrel was distributed to the nearest soil and that it became the cause of disease for some of the seed sown there. Plot F was not subject to the effect of the spread of the dust. We have seen that it was not dangerous to darnel; but plot E, farther from the barrel than any of the others, was also the most diseased. Plot D, designed to bear smut throughout, need not be considered here. The first pair of rows in plot C showed fewer signs of the disease than did the corresponding pair in plot E, the farthest from the barrel; and, for the same reason, plot G, as well as those of the first square, bore much less smut than all the others.

One will be even more inclined to believe, after I shall have reported on the following experiment, that the dust from the bunt balls, spread out over some of my plots before they were sown, and became there the source of infection of a large number of heads.

Perhaps I should pass in silence and be content to enjoy in secret the instruction it has afforded me. It would disclose in me a trait of contemplated malice and make me appear a dangerous neighbor. But it is to the public interest. Moreover, I am prompted only by ideas of helpfulness, even though I make mistakes. The damage I occasioned was not very great; moreover, it befell people who are no longer concerned with things here below.

The Reverend Carthusian Fathers have a piece of land at their door and hard by the little field I am cultivating. The day I sowed plots 67 and 68 and when, as a consequence, I had in my possession a large quantity of the dust from the bunt balls of diseased wheat to distribute in the seed furrows, the Reverend Fathers were sowing some wheat in their field. My work finished, I had a little of this dust left. I profited by the favorable occasion which presented itself, by using this dust in another tract than mine and thereby made a duplicate test.

On the evening when the workmen of the Carthusian Fathers had unhitched, I entered their field; I observed that part of the ground was yet unsown and that, according to all appearances, it would be sown on the morrow. I spread a little of the remaining bunt dust in an easily observable spot in this part not yet sown. The space within which the dust fell was probably 8 or 10 feet square. On the morrow, as I

had thought they would, they sowed in the rest of the field some wheat simply dipped in limewater and the whole lot was worked in to a considerable depth.

I lost sight of this experiment until the month of June, 1753, when I went to examine the plot belonging to the Reverend Fathers and recognized the little tract I had inoculated.

When I was about the put my measures to the test of finding it, it unquestionably made itself known by the bad condition of its crop of heads. The majority were attacked by smut; while, in the rest of the piece I could perceive almost no bunted heads. Brother Portier of the Carthusians had frequent occasion to examine this piece of wheat; it was under his very eyes. I called to his attention the little part infected with smut; he was struck by the great number of diseased heads presented to his view by such a small area and even more surprised to see it in this place. The cause of this phenomenon seemed, nevertheless, quite natural to the good Brother: an evil wind or the mist, said he positively, produced that spot. I sought not to disabuse him; perhaps I would have been unsuccessful had I attempted it.

The results of the large number of experiments the details of which have been given, afford, I believe the strongest evidence of the fact that I had set out to determine. I shall not, therefore, report the tests I made again with a view to confirming it, either the one in which I employed pure moss, as in the preceding year, or the one in which differently prepared soils were put in boxes and pots and sown to the same kind of seed or the same depth of soil, different seed according to the conditions under which I sowed it, or by putting in rows of isolated plants wherever my consignment of ground permitted. I was now wearied at seeing the surprising effect of some particles of dust and of always seeing the same result in a thousand places.

It will suffice here to assure you that the smallest tests have always agreed more or less perfectly with the experiments carried out on a large scale and that the smut disease, always contagious through its dust, has never failed to be controlled, or at least greatly reduced, by the remedies employed. I shall close by reporting two experiments the result of which, at first, seemed marvelous in comparison with

all we had thus far seen, and appeared far indeed from those of the second experiment.

I took the first box that fell into my hands. I divided it into two equal parts by means of a little wooden partition, which fitted quite well and prevented communication of one part with another. One side of this box was filled with new soil that I had taken from that thrown out of a town ditch. The other side was filled with this same kind of soil into which had been mixed some bunt spores at the rate of 1 to 5 (1 part bunt to 5 parts of soil). I sowed some clean wheat in the two parts of the box. The one produced only healthy heads; the other produced a large number of bunted heads.

I here present the second experiment. On the 19th of March, 1753, I transplanted in a pot, filled with soil that I was sure of, five wheat plants taken from a place, which, in my opinion, should produce only healthy plants. I watered these plants twice each day and until their condition was assured, with water into which I had thrown an abundance of smut and into which I infused a large amount of contaminated straw and bunt balls. To my great astonishment, none of these plants suffered from a watering that seemed must carry enough infection throughout to cause the plants to perish.

This singular fact shows that one must not make deductions too hastily. I considered my plants as lost in watering them thus with infested water, the condition of which was produced only by that same dust so fatal to the seed. But it is to be presumed that the wheat roots, once they have attained a certain stage of vigor, are safe against smut contagion, though it appears they are susceptible while they are still tender and delicate. In fact, it may be recalled that in plot 68 there were many bunted heads. This phenomenon doubtless came about only as the otherwise healthy roots came in contact, through growth, with the trail of smut that was only a little more than half an inch from the seed and there absorbed the poison, which the roots transmitted to the stem.

Continuation of the Second Project.

In proposing to the natural philosophers a subject so important as that of explaining the cause of the destruction of the wheat grain, etc. the Academy had two objects in view: Public welfare and the promotion of natural philosophy. It would, no doubt, be praiseworthy to satisfy both

perfectly. But, wherever, in my research, my efforts have not gone that far, we must admit that some helpful facts, althought a bit obscure, will, nevertheless, appear to have some value. The point under consideration possesses the particular merit of being of interest to all men as citizens and concerns only a small number among them as scholars. I shall, therefore, omit here any purely philosophical discussion. I shall, therefore, thenceforth avoid any and all purely scientific discussion. I shall risk no opinion that partakes of doctrine concerning the basic cause of the principal disease of wheat. It [the cause] may be of such nature, even, as to escape our recognition. In such respect it would have something in common with several causes of disease in the animal kingdom that cannot with certainty be determined, although the diseases, themselves, may be known to a certain degree and are often successfully treated.

It must suffice me, in considering the question from the practical side only, to be able to show in a positive manner and on the testimony of many experiments, (*1*) *that the common cause, the abounding source of bunted wheat plants resides in the dust of the bunt balls of diseased wheat; that the clean healthy seed, inoculated with this dust, receives through rapid contagion and a very intimate communication the poison peculiar to it; that it transmits the poison to the kernels of which it is the origin; that these kernels, once infested become converted into a black dust and become for others a cause of disease; that the culms, themselves, that bore the bunt heads contain something pestilential for the seed that lies near them and on which they germinate.*

In the second place, I am able to assert, *that the treatments I employed have protected the most heavily infested seed against the effects of the contagion, and that the success of these treatments has been so much the more positive even though the seed, on being treated, always retained the large quantity of smut with which it had been inoculated.*

Even though one judge my observations favorably and adopt all the facts that I have collected, one may still find oneself baffled by certain difficulties that it is well to disclose here. I shall have occasion, in seeking to resolve them, or at least in making them look like ordinary difficulties, to insist on certain points that have only been touched in passing and that, nevertheless, merit some discussion.

First Difficulty. Why, one may ask, in a field—sometimes, even, in a plot—where only clean, selected and sometimes treated seed had been sown—are there nearly always some bunted heads to be seen?

I shall first reply that no matter what attention one may give to the selection of the seed it is all but impossible not to find some kernel either wholly bunted or only partly so or some sound kernel that had made contact with a diseased one. In a sheaf that one has, oneself, prepared from the finest heads, there may be one containing two or three bunt balls. The dust that ruins these kernels, suffices to contaminate many others when the sheaf is threshed. It may be recalled in speaking of the clean selected wheat used in my 1752 sowings, that I noticed, in spite of my precautions, some bunt balls slip in. Could not their poison be as virulent for the healthy kernels nearest them?

In the second place, I shall say that it is not clearly determined that in our climate bunt originates by itself and independently of any contagious agent. A certain fact, determined by my own observation, is that one of the most skillful farmers of the neighborhood of . . . who devotes scrupulous attention to the choice of his seed never has any diseased wheat, although the different parts of the ground he has improved are situated some considerable distance apart and it would thereby seem should afford some opportunity for variation in the nature of the crop. A fact as certain as the first is that the farmer whose bunt-infected field was alongside mine in 1752 has had his wheat constantly damaged in several consecutive years through the practice (1) of sowing only the same seed—seed that, the first time grown by this farmer, was damaged by smut; (2) of using no other manures than those with which his straw was incorporated; and (3) by thus perpetuating bunt in several pieces of ground he had under cultivation.

Does not the condition of the wheat of these two farmers, so different for the sole reason that the one sowed only selected seed and the other for a long time had used only contaminated seed, lead you to suspect that smut is not of spontaneous origin, that it is an exotic disease in our clime, and does not occur here except through contagion?

In that respect it is similar to a formidable and unfortunately too common disease, which, from the time it ap-

peared in Europe, passed for an epidemic disease caused by bad atmospheric conditions; whereas today it is regarded as very certain that it does not develop at all unless the virulent principle is provided by a host already infected with the same disease and that, once this principle is eliminated, the disease is never produced by any other circumstance.[2]

In reading the work of the scholarly physician whom I cite here, one will note a sort of analogy between the disease of which he speaks and that of bunted wheat. For example, with reference to the former, has it not been observed that an ordinary disease principle is not invariably peculiar to such and such host; or that, if this principle, without exception, produces its effect, it is more widespread and marked on certain hosts than in others? Is it a question of the latter disease? One observed that not all the wheat grains that are uniformly blackened with smut are affected by the virus and that among the number of those that are affected, some produce completely bunted plants and others produce plants in which the disease extends only to a part of the heads or to some kernels within a single head. Whether one finds the virus innocuous or only partly effective or, again, wholly so, undoubtedly always depends relatively on the peculiar nature of the inoculated kernels and on the special disposition of the germ. Here we have a condition not unlike that peculiar to human beings exposed equally to attack by pests and without protection against them. Whether this or that terrible plague produce the effect of which it is capable or only a mild attack or none of any noticeable degree, will depend relatively on the condition of the body at the time or on its particular temperament.

M. Austre.

Second Difficulty. If the straws drawn from bundles con-

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[2] Ignorabatur luem Veneream, cum in Europâ primum apparuit; contagione traduci usu Veneris, aegrotis scilicet modum quo infecti erant cautè dissimulantibus, ut flagitia celarent; vel forsan ne suspicantibus quidem posse tam gravem morbum sola Venere contrahi, ut pote quae contagionis ratio planè insolens videretur. Indè adeò factum est ut Medici, qui illo tempore florebant summâ consensione crediderint ut jam supra dictum fuit, novum hunc morbum, more caeterorum pestilentium, Epidemicum, seu Popularem esse et induci proinde à causâ extremâ et communi qualem alii esse putabant noxium Astrorum influxum, vel Planetarum aspectum; alii verò insalubrem aëris statum, quem pluviae vel inundationes attulissent. . . . Sed pervicit tandem nota rec veritas ac jamdudum certâ, constanti, indubitatâ experientiâ innotuit, et unanimi Medicorum testimonio compertum est luem Veneream non concipi, nec errore dioetae, nec aëris vitio nec ullo rerum non naturalium abusu, nec spontanea humorum labe; *sed unicâ tantum ratione propagari si ab aegroto in sanum communicatione tradatur.* De morbis Venereis. Tom. I, pag. 119 & 120.

taining much smut are dangerous to seed sown in a dry condition, why do they occasion so little damage when buried or mixed with manure?

Assuming that the supposedly contaminated straw were impregnated with the kind of virus contained in the bunt dust, one should not be at all surprised by the condition of putrefaction to which they were found reduced in the manure. They lost the major part of it and, therefore, became less dangerous to the seed. In fact, we have seen that, following the use of a sort of composite manure, where rotting of the contaminated straw was incomplete, the kernels produced a large number of bunted heads.

But, if there be reason to hesitate in regarding the straw as susceptible to any virus, I shall still have a plausible explanation to give for the proposed difficulty.

In spite of the precaution I exercised in preventing any smutted heads from being incorporated with the straw I was not, however, able to prevent the straw from becoming contaminated with bunt dust when it was threshed and at which time the bunt balls were removed from the heads, and only the straw was saved. It is very evident that this fine dust was attached to the straw, for, when the straw was chopped to pieces, there arose from it a dust that entered the workman's throat and caused him to cough frequently.

That granted, I shall state that if the dry straw that was spread in the furrows was really contaminated with a little of the dust from the bunt balls, it is natural and in accord with numberless experiments that the soundest seed will suffer from proximity to such contaminated straw. But it is no less natural to assume that if the straw under suspicion possessed, in itself, nothing harmful and could harm the seed only through an extraneous dust with which it was contaminated, the dung mixed with this straw was more or less capable of absorbing this dust and of arresting its action.

In thus recognizing no bad intrinsic quality in the culms that had borne the bunted heads, one might very easily explain why, in that part of the tract infected with smut in 1752, the stubble I left there and on which I sowed some wheat produced no baneful effect. After all, leaving out of our consideration of the subject the possible fact that one would have nothing to fear from the stubble remaining in a

field where a large amount of smut had been harvested, it is quite inconsequential to a farmer whether his straw be dangerous in itself or whether it be only the cause of the dust with which it was contaminated. It is impossible for him to dodge the fact that, when he threshes the sheaves, the contaminating dust spreads to the healthy straw as well as the diseased. The only precaution this farmer is prepared to observe is that of not using manures with which suspected straw has been mixed or to use only a manure in which such straw had been completely rotted.

Third Difficulty. It has been suggested, and it is apparent, that the virus contained in the dust with which the seed was inoculated penetrated the seed, attacked the germ, infected the roots, became more or less active with the growth of the plant and became manifest in all its vigor, at first in its effect on the stamens and, finally, on the developing kernel. How can a wheat plant, within which this poison resides 7 or 8 months, carry on with only certain outward symptoms manifested by the culms and leaves (and, yet, a darnel plant, as we have remarked, shows none whatever in either the one or the other), while the kernels produced by the plant are found ruined before they are fully formed?

I shall not make a direct answer to this question; I shall limit myself to an attempt to establish the existence of a virus in the plant, although its presence there may be scarcely evident and all the disorder seems concentrated within the kernel itself, by calling attention to the fact that this, which appears to be peculiar to wheat plants attacked by bunt, is quite common in several of the diseases of animals. I shall cite only one of them. The parts of the human body have between them too many points of union. The solids and liquids of the body react with so much consonance that, in a man, attacked by gout, the entire mechanism does not suffer from the vicious causal principle. It is, however, in the joints where this malady has its location; rarely does it manifest itself elsewhere.

Fourth Difficulty. Why do not the remedies, with their very salutary effects on the seed *en masse*, extend universally to each and every grain rather than leave some vestiges of the disease?

One will be less surprised that the efficacy of the preparations (treatments) does not extend completely to all the seed when it will have been noted that I sought to bring into contact in the treated grain both the cause of the disease, such as has never been seen, and the remedy designed to prevent it. Surely, nobody would presume to bury his seed in bunt dust nor apply a remedy to seed thus inoculated. This test was, however, useful to me. I performed it in order that I might the more truly judge of the efficacy of the remedy. A farmer might never have occasion to employ it except on lightly inoculated seed and even then he might in the very beginning remove the little smut that adhered to the brush of the kernel by the simple act of washing the seed in ordinary water. Is it not fair to presume that the treatments will produce a more universal effect in the latter instance than they would in the special experiment I performed?

I should again note that in the best selected seed some kernels are sometimes found that, though apparently sound, are inwardly partly sound and partly bunted. However, they are quite capable of germination and subsequent production. The grain produced by plants grown from such seed suffers without fail from the defect of its origin.

The remedies I have employed have had their effect only insofar as the cause of the disease was present on the surface of the seed. Are these remedies sufficiently potent in those instances, still very few and concerning which one ought to be a bit concerned, where the cause must be met even within the interior of the seed? This is something I have not yet demonstrated. Moreover, in the large number of remedies I have employed in trying to prevent smut, there will be found, unless I am much mistaken, some one remedy of specific value against this disease to the extent that it nearly wipes it out.

It is true that three of these remedies, although apparently simple, and in the order of those that have given me such good success, have been so violent as to kill a large number of the diseased kernels to which I have applied them. But it is, nevertheless, well to have at hand some remedies capable of penetrating beyond the pericarp. Once their strength becomes known, it will be easy so to handle them as to render them salutary to the seed and to dilute

them to the point where the germ will not be affected and where the internal cause of the disease will be met.

If success authorizes me to recommend the remedies that I have latterly employed, this is a second benefit that will result from them and that it was necessary for me to keep in mind while carrying on my study of preventive measures. Most of these remedies will cost nothing; others will cost little; all will be easy to apply. I have been well aware of the fact that the farmer who is sometimes poor at figures and who does not willingly depart from his practices might not welcome even a slight expense, or he might be embarrassed by a preventive measure requiring too much work. I have, therefore, tried to follow his sense of economy, to approximate his practices, and to be useful to him without inconveniencing him.

First Project

PROJECT OF THE PLOT USED

First Division
Pigeon Manure

Oct. 16	Oct. 22	Oct. 27	Nov. 3	Nov. 10	Nov. 22
Wheat blackened with bunt	Naturally contaminated	Artificially inoculated	Naturally contaminated	Artificially inoculated	Naturally contaminated and artificially inoculated
1	5	9	13	17	21
Salted and limed	Salted etc.	Salted	Salted	Salted	Salted
2	6	10	14	18	22
Clean	Clean	Clean	Clean	Clean	Clean
3	7	11	15	19	13
Treated with Saltpeter and lime	Limed	Saltpetered etc.	Limed	Saltpetered	Limed
4	8	12	16	20	24

One hundred and eight feet

Second Division
Sheep Manure

Oct. 16	Oct. 22	Oct. 27	Nov. 3	Nov. 10	Nov. 22
Artificially inoculated	Naturally contaminated	Artificially inoculated	Naturally contaminated	Artificially inoculated	Naturally contaminated and artificially inoculated
25	29	33	37	41	45
Salted	Salted	Salted	Salted	Salted	Salted
26	30	34	38	42	46
Clean	Clean	Clean	Clean	Clean	Clean
27	31	35	39	43	47
Saltpetered	Limed	Saltpetered	Limed	Saltpetered	Limed
28	32	36	40	44	48

One hundred and eight feet

Third
Fecal

Oct. 16	Oct. 22	Oct. 27
Artificially inoculated	Naturally contaminated	Artificially inoculated
49	53	57
Salted	Salted	Salted
50	54	58
Clean	Clean	Clean
51	55	59
Saltpetered	Limed	Saltpetered
52	56	60

One hundred

TOTAL LENGTH OF PLOT

N THE EXPERIMENT OF 1751-1752

ivision atter			Fourth Division Horse and Mule Manure						Fifth Division Natural Soil					
v. 3	Nov. 10	Nov. 22	Oct. 16	Oct. 22	Oct. 27	Nov. 3	Nov. 10	Nov. 22	Oct. 16	Oct. 22	Oct. 27	Nov. 3	Nov. 10	Nov. 22
tu-lly n-ni-ted	Artificially inoculated	Contaminated and inoculated	Artificially inoculated	Naturally contaminated	Artificially inoculated	Naturally contaminated	Artificially inoculated	Contaminated and inoculated	Artificially inoculated	Naturally contaminated	Artificially inoculated	Naturally contaminated	Artificially inoculated	Naturally contaminated
1	65	69	73	77	81	85	89	93	97	101	105	109	113	117
ted	Salted	Salted	Salted	Salted	Salted	Salted	Salted	Salted	Salted	Salted	Salted	Salted	Salted	Salted
2	66	70	74	78	82	86	90	94	98	102	106	110	114	118
ean 3	Clean 67	Clean 71	Clean 75	Clean 79	Clean 83	Clean 87	Clean 91	Clean 95	Clean 99	Clean 103	Clean 107	Clean 111	Clean 115	Clean 119
ned	Saltpetered	Limed	Saltpetered	Limed	Saltpetered	Limed	Saltpetered	Limed	Saltpetered	Limed	Saltpetered	Limed	Saltpetered	Limed
4	68	72	76	80	84	88	92	96	100	104	108	112	116	120

d eight feet | One hundred and eight feet | One hundred and eight feet

IVE HUNDRED AND FORTY FEET

SECOND PLAT PLAT OF GROUND USED IN

Clean wheat without manure 8 bunted heads, four [loose]-smutted. 1	Clean wheat without manure. 5 bunted heads, 4 [loose]-smutted. 6	Clean wheat without manure. 11 bunted and 2 [loose]-smutted heads. 11	Clean wheat without manure. 6 bunted heads, 2 [loose]-smutted. 16	Clean wheat without manure. 1 bunted head, 4 [loose]-smutted. 21
Clean wheat, horse manure made of clean straw. 14 bunted heads, 2 [loose]-smutted. 2	Horse manure made of clean straw. 6 bunted heads, 1 [loose]-smutted. 7	Horse manure and clean straw. 2 bunted and 3 [loose]-smutted heads. 12	Manure comprising sheep manure and healthy straw. 19 bunted heads, 3 [loose]-smutted. 17	Manure consisting of cow dung and clean straw. 2 bunted heads, 3 [loose]-smutted. 22
Clean wheat, horse manure made of clean straw. 4 bunted heads and 2 [loose]-smutted heads. 3	Horse manure made of straw of doubtful freedom from bunt. Fewer bunted heads than in the plots thus fertilized, 4 [loose]-smutted heads. 8	Horse manure and healthy straw. 1 bunted head and 3 [loose]-smutted heads. 13	Manure comprising sheep manure and healthy straw. 94 bunted heads, 4 [loose]-smutted. 18	Manure comprising cow dung and clean straw. 32 bunted heads, 7 [loose]-smutted. 23
Clean wheat, horse manure made of straw of doubtful freedom from bunt. 48 bunted heads, 2 [loose]-smutted. 4	Manure composed of sheep droppings and clean straw. 1 bunted head, 2 [loose]-smutted heads. 9	Horse manure and suspected straw. 11 bunted and 6 [loose]-smutted heads. 14	Manure comprising sheep manure and suspected straw. 825 bunted heads, 3 [loose]-smutted. 19	Manure comprising cow dung and suspected straw. 29 bunted and 4 [loose]-smutted heads. 24
Clean wheat, horse manure made of straw of doubtful freedom from bunt. 49 bunted heads, 3 [loose]-smut. 5	Manure composed of sheep droppings and straw of doubtful freedom from bunt. 10 bunted and 1 [loose]-smutted heads. 10	Horse manure made of suspected straw. 22 bunted heads. 15	Manure comprising sheep manure and suspected straw. 879 bunted heads, 2 [loose]-smutted. 20	Manure comprising cow dung and suspected straw. 30 bunted and 3 [loose]-smutted heads. 25
Healthy darnel on clean soil. 141 healthy heads, 1 ergot, no bunt. A	Healthy darnel contaminated with the dust of bunted darnel and treated with saltpeter and lime. 240 healthy heads, 1 ergot, 0 [loose] smut. B	Healthy darnel in soil inoculated with bunt of wheat. 1 ergot, 0 bunt. C	Darnel inoculated with bunt of wheat. 230 sound heads, 0 bunt. D	Sound darnel, contaminated with darnel bunt. 224 sound heads 96 bunted heads 320 heads E

First part of the plat worked with the plow; 116 feet long by 12 feet wide.
Clean wheat, well washed and dried in the sun, although the heads from which this grain came were selected from very sound sheaves.

RESULT
Very good: Bunted heads extremely rare.

WIDTH 48 FEET

LENGTH OF

Clean wheat without manure. Sound straw cut-in-pieces. 2 bunted heads and 1 [loose]-smutted head. 26	Clean wheat without manure. 5 bunted and 3 [loose]-smutted heads. 31	Clean wheat without manure. 2 bunted and 2 [loose]-smutted heads. 36	Clean wheat without manure. 3 bunted and 3 [loose]-smutted heads. 41	Clean wheat; without manure. 3 bunted heads. 46
Suspected straw. 27	Clean wheat, dampened with mucilage and inoculated with bunt. At least ⅓ of the heads, bunted. Poor (thin) stand. 32	Clean wheat, inoculated with bunt, dampened, treated with salt-lime solution without losing any of the bunt. 10 bunted and 3 [loose]-smutted heads. Quite good. 37	Clean wheat, inoculated with dry bunt and buried in the dust. 566 healthy plants, 491 diseased plants 1057 2026 sound heads 1687 bunted heads 3713 heads 42	Clean wheat, contaminated with darnel dust, inoculated with wheat bunt and buried in the dust. Affected like 42, 43, 44, & 45 but to a less degree. More delay in the evidence of bunt. 47
Suspected straw. 28	Clean wheat, dampened with mucilage and inoculated with bunt. ⅓ the heads bunted, five heads of loose smut. 33	Clean wheat, inoculated with bunt, moistened, treated with salt-lime solution in this condition without loss of bunt. 8 bunted heads, 3 or 4 [loose]-smutted. Excellent. 38	Clean wheat, inoculated with dry bunt and covered at a depth of one to two inches. ⅓ or less of bunted heads, 6 [loose]-smutted. Poorer than the preceding. 43	Clean wheat, contaminated with darnel dust, inoculated with bunt, and buried at a depth of 1 or 2 inches. 778 healthy plants 365 diseased plants 1143 plants 2194 sound heads 898 bunted heads 3092 heads 48
Sound, threshed heads and sound kernels. 29	Clean wheat dampened with fish glue and inoculated with bunt. ⅓ or nearly ⅓ the heads bunted. 34	Clean wheat, inoculated with bunt, dampened, treated with saltpeter and lime without loss of bunt. 3 bunted heads. Some [loose] smut. Excellent. 39	Clean wheat, inoculated with dry bunt and buried at a depth of 3 or four inches. ⅓ of all heads bunted, 5 [loose]-smutted. Not so good as the 43rd plot. 44	Clean wheat, contaminated with darnel dust, bunted, and covered at a depth of 3 or 4 inches. ⅓ of the heads bunted. 49
Threshed bunted heads and questionable kernels. 30	Clean wheat dampened with fish glue and inoculated with bunt. ⅓ of the heads bunted. 35	Clean wheat, inoculated with bunt, dampened and then treated with saltpeter and lime without loss of bunt. 84 bunted heads, 3 or 4 [loose]-smutted heads. Excellent. 40	Clean wheat inoculated with dry bunt and buried at a depth of 5 or 6 inches. 70 sound plants 211 diseased plants 281 plants 331 sound heads 938 bunted heads 1269 heads 45	Clean wheat contaminated with powder of darnel, inoculated with bunt and covered at a depth of 5 or 6 inches. 318 sound plants 344 diseased plants 662 plants 903 sound heads 998 bunted heads 1901 heads 50
Sound darnel, contaminated with darnel bunt and washed. 217 sound heads 129 bunted heads 346 heads F	Darnel collected in a field where there was much darnel attacked by bunt. 463 sound heads 1 bunted head 464 heads G	Clean rye in non-contaminated soil. 175 healthy heads. H	Clean rye in soil inoculated with wheat bunt. 31 sound heads, 1 ergot. I	Clean rye, inoculated with wheat bunt. 144 sound heads, 4 heads affected with ergot. K

First division containing 4 rows of clean barley.

No disease.

Six feet.

Twelve feet.

Second part of plat, worked with the plow; same dimensions as the first. Same seed as in the first part, with this difference, namely, that it had been heavily inoculated with bunt on Oct. 4, left, thus contaminated, enclosed in a box, until the 24th of Oct., when it was well washed, exposed to the sun to dry and sown the following day. One could see by the aid of the low power of a microscope, however, some particles of bunt in the brush of the washed kernels which the washing did not remove.

RESULT

Quite good: A large number of bunted heads.

THE PLAT

Continuation of the Second Plat

Clean wheat 8 bunted heads, 2 loose-smutted. 51	4 bunted heads. 56	6 bunted heads. 61	2 bunted heads. 66	3 bunted heads. 71
Clean wheat, on horse manure made of suspected straw. 40 bunted heads, 6 loose-smutted. 52	Clean wheat, inoculated with bunt, No. 4. 2/5 of the heads bunted. One loose-smutted. 57	Beardless wheat from a field infected with bunt. A few bunted heads and some loose smut, enough, indeed, to stigmatize this kind of wheat. 62	Clean wheat, in furrows the bottom of which had been dusted with bunt. Many bunted heads. 67	Clean wheat, salted and limed. Some bunted heads, otherwise excellent. 72
Clean wheat, on horse manure made of suspected straw. Almost like the 52nd. 53	Clean wheat, inoculated with loose smut. Quite a large number of bunted heads. Three loose-smut heads. Quite good. 58	Beardless wheat from a bunt-infected field. Same observation as in plot 62. 63	Clean wheat, in rows where a mere line of bunt had been run approximately ½ inch from the seed. Many bunted heads. 68	Clean wheat, saltpetered and limed. Some bunted heads. Otherwise very good. 73
Clean wheat, no manure. Four or five bunted heads. 54	Clean wheat inoculated with barley smut. Several bunted heads. Two loose-smut heads. Quite good. 59	Bearded wheat from a field infected with bunt. Same observation as in the two preceding plots. 64	Beardless wheat, dusted with bunt. 1641 sound heads 1476 bunted heads 3117 heads 69	Clean mouse wheat. No bunted heads. 74
Wheat from partly smutted, partly sound heads, picked out by hand and washed. Very few bunted heads. One loose-smutted. 55	Clean wheat inoculated with oat smut. Few bunted heads. Two loose-smutted. Excellent. 60	Bearded wheat, from a field infected with bunt. Same observation, etc. 65	Bearded wheat inoculated with bunt. 1624 sound heads 1705 bunted heads 3329 heads 70	Mouse wheat, inoculated with bunt. A few bunted heads. 75
Rye dusted with the powder of ground ergots 286 healthy, 8 ergoted heads. L	Clean common barley in noncontaminated soil. No disease M	Clean common barley in furrows the bottoms of which had been dusted with bunt of wheat. No disease. N	Clean common barley dusted with bunt. No disease. O	Clean common barley, moistened, and then blackened with bunt. No disease. P

Second division containing 4 rows of 4-row barley inoculated with bunt of wheat.
No disease
6 feet
←——→

360 FEET

Continuation of the Same Plat

Contaminated Carthusian wheat, washed. Several bunted heads. 76	Clean mouse wheat. Several aborted heads. 81	Clean mouse wheat. A single blighted head. 86	Clean mouse wheat. Some aborted heads. 91	Clean mouse wheat. Several heads in an aborted condition. 96
Contaminated Carthusian wheat, saltpetered and limed. Very few bunted heads. 77	Spring wheat, picked by hand, unwashed. Some bunted heads. Some loose-smut heads. Mediocre. 82	Clean common barley. Six smut heads. 87	Clean spring wheat. 1484 sound plants 8 diseased plants 1492 plants 3124 sound heads 15 diseased heads 3139 heads 92	Clean oats. 4 or 5 smutted panicles. 97
Contaminated Carthusian wheat, salted and limed. Very few bunted heads. 78	Same spring wheat, dry, inoculated with bunt. 1/5 the heads bunted. Some loose-smut. Mediocre. 83	Clean common barley, dry, inoculated with spores of smut of barley. 39 smutted heads. 88	Clean spring wheat with suspected wheat straw. 2025 sound plants 676 diseased plants 2701 plants 3215 healthy heads 1165 bunted heads 4380 heads 93	Oats, dry, dusted with spores of oat smut. Like the preceding. 98
Contaminated Carthusian wheat, nontreated. A large number of bunted heads. 79	Same spring wheat, moistened with mucilage solution and dusted with bunt. 3/5 of heads bunted. Some loose smut. Mediocre. 84	Clean common barley, dry, dusted with bunt of wheat. Six loose-smutted. 89	Spring wheat, dry, dusted with bunt. 1/2 the heads bunted. Several loose-smutted. Weak and short in spots. 94	Oats, dry, dusted with bunt. Like the preceding. 99
Some contaminated Carthusian wheat, nontreated. A large number of bunted heads. 80	Miracle wheat, without treatment. No disease, excellent. 85	Clean common barley, dry, inoculated with barley smut and treated with saltpeter and lime in this condition. 35 heads of smut. This and the three preceding plots were very fine. 90	Spring wheat, blackened with bunt, dry, treated with saltpeter and lime in this condition. Few bunted heads. Some loose-smutted, Seven or 8 inches taller than the preceding plot. 95	Oats, dry, inoculated with oat smut spores and treated with saltpeter and lime. Like the preceding. These last four plots were uniformly excellent. 100
Clean common barley, dusted with common-barley smut. No disease. Q	Healthy common barley, moistened and dusted with spores of common-barley smut. No disease. R	Healthy common barley, blackened with bunt, treated with saltpeter and lime in this condition. No disease. S	Mouse wheat moistened with mucilage solution and blackened with bunt. Several heads smutted but by no means so many as in a plot of winter wheat dusted with bunt. T	Clean mouse wheat. 1 or 2 bunted heads. Some crinkled plants. V

Third part of the plat worked with the plow, having the same dimensions as the two preceding parts.

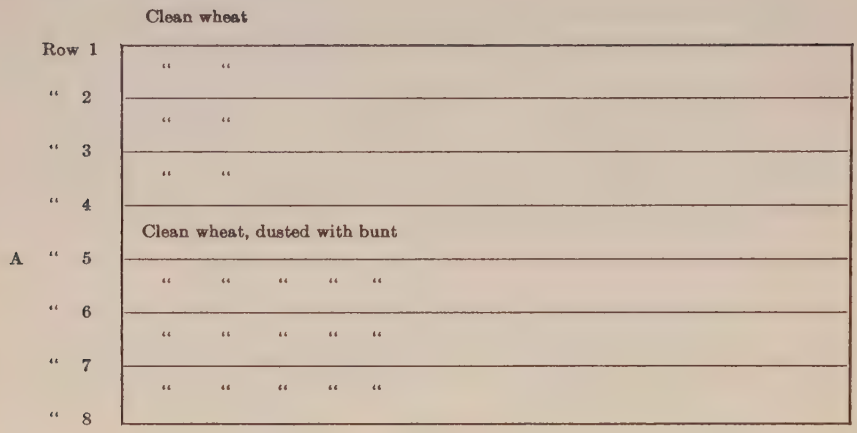
The same seed as in the Second part, with this difference that, after having washed it superficially, it was dusted while still moist, with new bunt, in order that it might the better adhere to the seed.

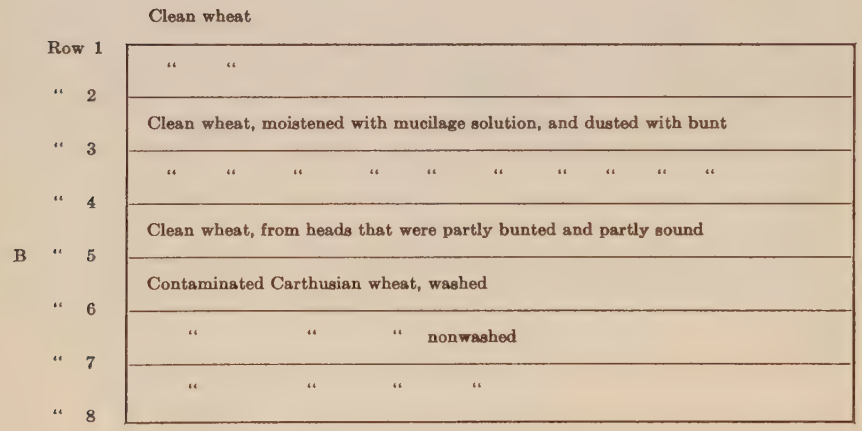
RESULT

A large number of blighted plants. Many weak and short culms. A prodigious quantity of bunt-infected plants. In seizing a handful of heads, one could find scarcely three or four sound ones.

OVER ALL

Third Plat

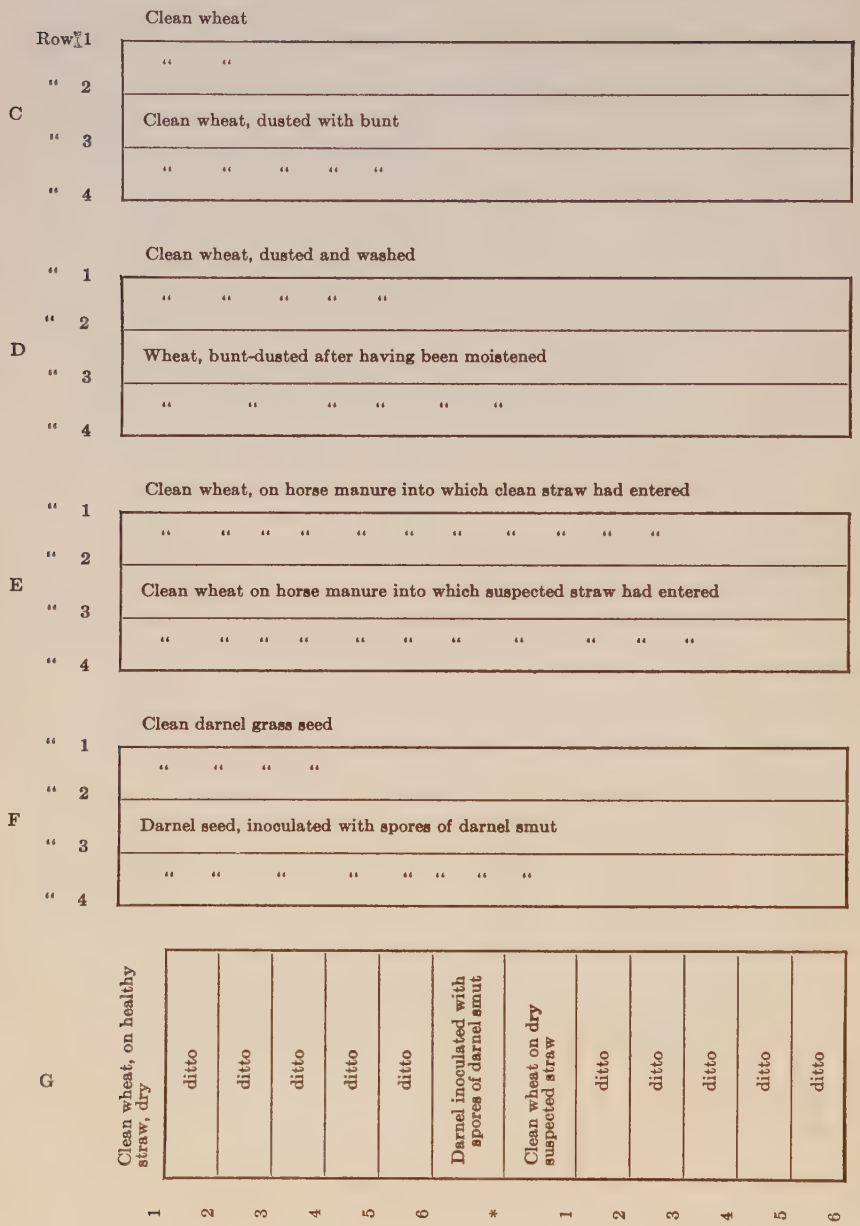

Clean wheat
Row 1
" 2
" 3
" 4
A " 5
" 6
" 7
" 8
" "
" "
" "
Clean wheat, dusted with bunt
" " " " "
" " " " "
" " " " "


Clean wheat
Row 1
" 2
" 3
" 4
B " 5
" 6
" 7
" 8
" "
Clean wheat, moistened with mucilage solution, and dusted with bunt
" " " " " " " " " "
Clean wheat, from heads that were partly bunted and partly sound
Contaminated Carthusian wheat, washed
" " " nonwashed
" " " "

Results of the two plats of the First Square

	Healthy wheat plants	Sound heads produced by these plants	Diseased wheat plants	Bunted heads produced by these plants
Row 1	91	478	3	16
" 2	113	427	1	4
" 3	131	482	1	3
" 4	123	511	1	9
A	458	1898	6	32
" 5	29	142	67	369
" 6	27	115	93	443
" 7	32	124	94	399
" 8	24	96	79	369
	112	477	333	1580
" 1	88	366	9	29
" 2	107	545	4	23
	195	911	13	52
" 3	29	155	87	493
" 4	33	156	81	479
B	62	311	168	972
" 5	98	507	8	45
" 6	101	409	7	36
" 7	72	249	32	127
" 8	67	271	49	203
	139	520	81	330

Fourth Plat. Plan of the Second Square



Result from the five plots of the Second Square

		Sound heads	Bunted heads
	Row 1	461	25
	" 2	431	52
C		892	77
	" 3	43	429
	" 4	78	602
		121	1031
	" 1	233	562
	" 2	235	371
D		468	933
	" 3	35	457
	" 4	48	702
		83	1159
	" 1	542	153
	" 2	404	91
E		946	244
	" 3	361	151
	" 4	411	284
		772	435
	" 1	343	2
	" 2	484	11
F		827	13
	" 3	22	455
	" 4	39	444
		61	899

G	Sound heads	Bunted heads
	97	2
	81	0
	131	2
	136	0
	165	5
	162	2
	772	11
	11	110
	143	14
	109	22
	133	27
	127	14
	145	27
	163	18
	820	122

CONTINUATION

OF THE

EXPERIMENTS and REFLECTIONS

RELATIVE *to the Dissertation on the cause of the corruption and smutting of the kernels of wheat in the head, and on the means of preventing these unfortunate results,*

WHICH, in the judgment of the Royal Academy of Literature, Science and Art of Bordeaux, won the Prize.

By

M. TILLET, *Director of the Mint of Troyes*

IN PARIS

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CONTINUATION
OF
EXPERIMENTS
AND
REFLECTIONS
ON
the Cause of the Corruption and Smutting of the Kernels of Wheat in the Head and on the Means of Preventing these Untoward Circumstances

In the experiments that I performed in 1752 to 1753, and of which the second part of my dissertation presents the details, my principal object was to inoculate the purest and most select seed by blackening it with the spores from bunt balls obtained from smutted wheat, and thus produce a large quantity of smutted heads by sowing this inoculated seed, and thereby leave no doubt as to the hidden malignity of the dust with which I had blackened it.

In the experiments of 1753-54, which I am here about to describe, I had as my principal object, on the contrary, to employ only the most contaminated seed, that most laden with smut on leaving the fan and to subject it to different preparations in order to protect it against contagion, and to prevent any transmission of the disease to the heads that might come from such seed. In a word: I sought first to establish the principle of the disease; then, finally, my thought was directed toward preventive materials.

Among the remedies I shall mention, several have been wholly successful. The reader, moreover, is already aware of the fact that they possess advantages in which I was particularly interested—advantages that will leave the negligent farmer without any excuse. These remedies require little care; they cost nothing, or are held at a most moderate price.

When one knows the characteristics of the farmers, their sometimes poorly comprehended economies, their predilection for their own practices, and their hesitancy to re-

ceive anything not conceived by someone in their midst, one feels that it would be necessary, if he assure himself of their tractableness, to put them to no expense in acquiring something of value to them, to avoid requiring their departure from a certain round of operations to which they are committed and trick them, so to speak, in abandoning them, to look into scarcely any difference between the precautions one counsels them to observe and those to which they always have been constrained to practice.

The order I have followed in reporting my first experiments, being the simplest and most natural, I shall follow in describing these. I shall begin by giving a succinct account of the preparations required for the composition of the several fertilizers; I shall give the distribution of the plots where my tests were made; I shall tell of the different kinds of seed and even of the differently prepared lots of seed sown; I shall place each kind in its original order.

The observations that I had time to make while the wheat was growing, being the same as those presented in the body of my dissertation, I shall pass at once to the condition of the wheat already in head, to the outcome of my plots, to that obtained from a plot worked in accordance with the ordinary method, and to the deductions that I shall draw from it.

I shall refer next to some homemade experiments.

They were conducted in the same garden already under consideration: the account of them will be complete. From now on I shall not adhere to a certain precision in speaking of the operations carried on in the open country. It will be best if I render a more exact account of the results of experiments performed in the city and not limit myself to presenting simple estimates. Moreover, the experiments, on the whole, are as well adapted as I could desire to the small-scale tests I have observed daily and that it is easy for me to describe carefully.

The course I am following in this new supplement shows clearly that I do not write gracefully. My plan, in fact, is to present only the series of my operations, to make clear my thoughts, wherever they may lead me, and to develop my work well by placing the facts before the eyes of the reader precisely as they have passed under mine. First, he will find himself at the time of seeding. He will be a witness to my

preparations. He will follow me in my several seedings, and will see nearly all without yet understanding the outcome of them.

Then he will find himself arrived at the time when the ground is covered with heads of wheat and when their condition is fully established; he will recognize each plot, each parcel of ground, worked with the plow; he will recall the kind of seed sown in each and the sort of preparation accorded each lot of seed; he will examine the plants from the standpoint of their health or from that of the damage sustained by them and will judge without confusion concerning the efficacy of the remedies applied.

I admit that in the matter of observations this procedure is a bit slow and that it requires some steadfastness, the reader may be obliged sometimes to retrace his steps. But I beg of him to note that I touch upon a very important matter of natural philosophy and concerning which, until now, there has prevailed much obscurity; that I must, as a consequence, occupy myself less with the construction of the discourse than with the manner of presenting the subject matter with exactness; and that it would be difficult for me, to attain this end, not to make some repetitions under the necessity of having to relate a large number of operations and a multitude of facts that require a presentation of the minutest details.

We have seen from the results of the experiments of 1752-53 that in the third part of the little parcel of ground worked with the plow there was a prodigious quantity of bunted heads. It was the most severely infected sheaves of this particular canton that supplied me with the straw that was later to be incorporated in the manure and designated *suspected* and the seed to which I applied different preparations to protect it against every unfortunate circumstance.

On the 27th of August, 1753, I mixed with two lots of horse droppings equal amounts of chopped healthy straw and suspected straw, one lot containing chopped healthy straw only, the other lot only contaminated or suspected straw. I put each of these mixtures into a special vat, throwing in enough water to wet the straw down well; and I left them thus until the 15th of October, on which date I used my fertilizers.

On the 30th and 31st of August and the 6th and 12th of

September I mixed some clean straw, as well as some suspected of being contaminated, with equal parts of rich sheep manure, dry and reduced to dust. These mixtures I combined with cow dung. They were put in equal amounts into special vats and were watered, as were the other two. They were allowed to remain in this condition until I used them.

The lots of suspected straw incorporated with the different manures in 1753 did not contain all of the bunted heads, as did those which entered into the mixtures of 1752. However, they would have produced an obvious effect, for they came from an infested canton where sound heads were rare. Moreover, in a sheaf full of smut the few sound culms present, when threshed, would soon share the dangerous condition of those bearing smutted heads, for we infer, from all appearances, that the suspected straw, in itself, contains nothing contagious and is to be feared only because it is laden with the dust from the bunt balls.

It may be recalled that from the little field where my experiments were carried on in 1752 I reserved a part for those projected for the next year. This portion was 26 feet wide by 360 feet long. I left out two feet of the width and it will soon be seen how it was utilized.

In this reduced area I made 92 plots. They were arranged in 4 parallel series, 23 plots per series. Each plot was 15 feet long by 5 wide and a space of one foot was left between parallel series to serve as an alley lengthwise of the area. By making my plots three feet shorter than those of former years I was able to increase the number and I managed, moreover, to reserve a strip 15 feet wide across the entire width of the nursery, where I put in 8 rows of wheat and 8 of darnel.

These rows were sown during the course of the month of September; that is, from the first to the 29th. Following is the order in which I laid them out, the kind of seed sown, and the day on which it was sown. The wheat I used for this little parcel of ground was clean and carefully selected. The first row was sown on the first of the month; so also was the second row, after the seed had been blackened with the dust from the grains of bunted wheat.

Seed from the same lot, clean and smutted, sown on the 7th, in the order indicated elsewhere, comprised the 3rd and 4th rows.

On the 14th two additional rows were sown: I always observed the practice of sowing first a row of clean seed and then a row of smutted seed. The same operation obtained on the 22nd; the very choice wheat and the same blackened with smut were sown alternately.

The 9th and 10th rows were sown on the 24th. They contained equal quantities of clean darnel seed and seed of the same kind contaminated with the dust of bunted grains of wheat.

The wheat comprising the 11th, 12th, 13th, 14th, 15th, and 16th rows was sown on the 29th of September. This seed, sown sometimes clean, sometimes smutted, was sown in the same order as that already referred to, in relation to the first rows.

I did not continue these plots into the month of October, as it seemed but natural I should do: I would have returned at the regular time for sowing, since my object was to be forehanded. The first rows of this little nursery gave me opportunity to make a quite helpful observation, but different from the one I expected to make and that I needed. Rarely in the performance of experiments does one's labor answer to no purpose if one pursues it without distaste and always with his eyes open: one seeks a certain truth, it escapes him; one is not at all concerned about another, it impresses him.

The various fertilizers, the composition of which has been noted in the foregoing, were applied to the first 20 plots and, as in 1752, spread in the rows that had been opened to a depth of four or five inches.

The 1st and 2nd plots contained cow manure with which clean straw had been mixed.

In the 3rd and 4th I put the same kind of manure but with it I had mixed the suspected straw.

The manure consisting of horse droppings and clean straw was applied to the 5th and 6th plots.

To the 7th and 8th plots I applied manure consisting of horse droppings and suspected straw.

The rich sheep manure, with which I had mixed clean straw, was applied to plots 9 and 10.

The same sheep manure, mixed with supposedly smut-contaminated straw, was put in rows 11 and 12.

Plots 13 and 14 contained dry sheep manure mixed with clean straw.

Plots 15 and 16 were given the same sort of sheep manure mixed with suspected straw.

The 17th and 18th plots received an application of fertilizer consisting of sheep manure plus clean straw.

The 19th and 20th plots were fertilized with sheep manure mixed with supposedly contaminated straw.

My object having been again to see whether the supposedly contaminated straw, once incorporated with the dung of the different animals, exerted any harmful effect upon the seed proximate to it, it was thought best to employ in these first 20 plots selected seed so that it could not, in itself, be considered the active agent of any untoward circumstance.

I commonly limited myself to five rows of grain to each plot in order that the individual wheat plants might have every opportunity to grow and become vigorous. I put 6 of them in 12 successive plots in order to multiply the experiments and to afford room, when I desired it, for some varieties in one and the same plot to bring about certain differences between the 3 first rows and the 3 last ones.

The 21st plot contained rye. In the 3 first rows the seed was clean and nontreated. That of the 3 last was contaminated with the dust that had been produced by some crushed rye ergots.

I smutted some rye seed with the dust from some bunted wheat and sowed it in the 22nd plot.

The 3 first rows of plot 23 consisted of clean, nontreated 4-row barley. The 3 last rows were sown with similar clean seed smutted with the dust from bunted wheat grains. Likewise, all the rows of plot 24 were sown to similarly contaminated seed.

In the 3 first rows of plot 25 I sowed clean seed of darnel grass; in the remaining 3 rows I sowed seed of the same kind, inoculated with the dust of smutty darnel grains.

The 26th plot was a duplication of the 25th. Excepting the darnel seed sown in the 3 first rows of plot 32, the seed of the 6 remaining plots was always contaminated; that is to say, infested with the dust of the smut peculiar to the seed to which it was applied.

I sowed the first part of plot 27 to artificially inoculated

darnel seed after having moistened it, in this condition, with ordinary water containing nitre and dusted it with lime. Instead of nitre I employed common salt as a preservative of the darnel seed I sowed in the second part of the same plot.

The darnel, with which I sowed the three first rows of the 28th plot, I moistened with water that had leached through salsola (saltwort) ashes. That of the three last rows I dampened with leach from pearl ashes. [Chickweed]. Following is the simple method by which I obtained these detergent waters; it was the same as that that I employed in obtaining leaches from potash and the leached ashes of green wood.

I reduced to powder a pound of saltwort and a like amount of pearl ashes; I put each in two pints[1] of water, which I boiled for 30 minutes or thereabouts. I then filtered it all through gray paper. I drew from the soda leach about 3 *demi-setiers* of a very limpid solution; from the pearl ashes, on the contrary, I drew off a somewhat foul liquid. These solutions, as one may well judge, were more or less acrid and penetrating, depending on the amount of the specific salt each kind of ashes contained. After the evaporation of 3 *demi-setiers* of liquid which resulted from the leaching of each kind of ashes I found that, per pound, I had in precipitated salt:

	ounces	drams	grans i
of potash	8	0.5	0
of saltwort ash	4	5.5	18
of pearl ash	1	7.5	0
of green-wood ash	0	7.5	0
A pint of our domestic leach yielded also a precipitate of	0	2.0	0

The 29th plot was sown to darnel seed that I had moistened as much with the green wood-ash leach as with an ordinary homemade leach made by bleachers.

The 30th plot contained, in the first 3 rows, some darnel seed moistened with human urine voided in the morning, accumulated to a certain amount, exposed to the sun for 3 months and arrived at a certain degree of putrefaction. In the last 3 rows I sowed some darnel seed that I had moistened with ordinary water, into which had been mixed a little of spirits of nitre, and that I had subsequently dried by sprinkling over it some finely sifted quick lime.

[1] The Troye pint is greater by a fifth than that of Paris.

In the 31st plot I sowed equal amounts of contaminated darnel seed, one lot washed with ordinary water, the other with pure water. Both lots were then moistened with the leach from saltwort ashes and afterwards dried with lime dust.

The darnel sown in the first part of the 32nd plot was previously blackened with dust from bunted wheat grains. That sown in the remaining portion of the plot had been dampened with a mixture of 1 part of spirits of nitre and 7 parts of water, and afterwards dried with green-wood ashes.

The experiments performed in the 4 succeeding plots were repetition of those I had conducted in 1752 in plots 26, 27, and 28. They seemed to me to merit some consideration.

After having made 5 furrows, 4 or 5 inches deep, in the 33rd plot, the chopped wheat straw was placed in them. The straw was sound and mixed with it were the threshed wheat heads. This chopped straw was mixed with a little soil so that the roots of the growing wheat plants could find nourishment there. Selected seed was sown on this straw after which it was covered with soil.

The seed was the same for the 4 plots containing the different kinds of straw.

The 34th and 35th plots contained supposedly contaminated straw with heads. Not all of these latter had produced bunt balls; yet, in these plots there was subsequently observed the greatest number of them.

For the 36th plot I used only contaminated straw from which the heads had been removed.

I sowed Miracle wheat, blackened with bunt dust, in the 37th and 38th plots.

During harvest I was taken into one of the best wheat sections in the neighborhood of Troyes, and there I, myself, collected from standing grain some bundles of very healthy heads that, moreover, had not been near any diseased heads. From my little harvest I obtained seed that was as pure as it was possible to get and I sowed it, without treatment, in plot 39.

I used some of this seed also for the 40th plot after having moistened it with potash leach and sprinkled it with powdered lime.

The question now before us, for many of the plots, concerns the use of seed from the infected sheaves of which

I have already spoken. One may easily imagine that the seed obtained from such bundles would be covered with the contagious dust of the bunt balls and that mixed with the seed might be some bunt balls that the flail had not crushed.

In the 41st plot I sowed a portion of this wheat just as it was on coming from the fan and still black enough to soil the hand of the sower.

The greater part of this same grain was washed in ordinary water, care being exercised to change the water several times until it came away from the vessel in its natural clearness.

I sowed this washed seed in plot 42 and divided the plot off very neatly. From this plot, up to and including the 64th, it will be necessary always to infer use of the same seed washed in ordinary water before subjecting it to any other preparation.

Plot 43 contained seed that had been washed in ordinary lye water.

Plot 44 was sown with seed over which I had poured some lime water that was almost boiling and that contained no other ingredient.

The seed of the 45th plot was soaked in ordinary lye water and then dusted with lime.

For the 46th plot the seed was dampened with lye that had filtered through green-wood ashes, and was then dusted with lime.

I used lye from pearl ashes, and powdered lime, for the preparation of the seed for plots 47 and 48.

Lye from saltwort ashes, and powdered lime, were employed in preparing the seed for plots 49 and 50.

Potash solution, combined always with powdered lime, was used in treating the seed for plots 51 and 52.

The seed consigned to plots 53 and 54 was moistened with a saturated common-salt solution and then dusted with lime.

For the 55th and 56 plots there was this difference in the treatment of the seed, namely, that instead of a solution of common salt, I used a saturated solution of saltpeter.

The seed sown in plot 57 I soaked in slightly warmed putrefied cow urine. I made use of this same urine, combined with powdered lime, in the treatment of the seed sown in plot 58.

Putrefied, slightly warmed human urine was used both alone and finally absorbed by powdered lime in treating the seed for plots 59 and 60.

I soaked the wheat for plot 61 in lukewarm, thick, liquid manure.

That sown in the 62nd plot was dampened in the same way and then dusted with lime.

The seed in plot 63 was given a cold bath in a solution of 1 part spirits of nitre and 9 parts of ordinary water.

The seed for the 64th plot was washed in a warm solution comprising a mixture containing about an eighth part more of the saltpeter. The seed was subsequently dusted with lime.

From the preceding year I had a remnant of some selected wheat and a little of this same wheat, still covered with bunt dust. Plot 65 received the clean wheat, and plot 66 the contaminated seed.

After having washed this same seed quite superficially, inoculated since 1752, I dampened it with a potash solution, dusted it with lime, and sowed it in the 67th plot.

Without washing, some of this same seed, retaining all the contagious dust with which it was blackened was moistened with a saltpeter solution, dusted with lime, and sown in the next plot.

The 69th plot contained clean wheat obtained from sound heads that I had carefully picked out and collected from the plants while harvesting.

The 70th was of the same wheat, contaminated with the dust of smutty darnel seed.

The 71st plot was sown to some of the same seed used in the preceding plot but dampened with potash solution and dusted with lime.

The 72nd contained some of this same lot of seed inoculated with the dust of darnel smut, but dipped afterwards in saltpeter solution that was subsequently absorbed by powdered lime.

In plot 73 I repeated the experiment of communicating the smut disease through the newly developed roots of selected seed by putting a line of the bunt dust in the drill row where the seed was sown, but seven or eight lines [7/12 or 2/3 of an inch] distant from the seed, and by observing the precautions of which I spoke in the second part of my dissertation (page 112).

The furrow bottoms of the 74th plot were contaminated with the same contagious dust with which, in the preceding plot, I had made only some mere tracings. In this plot I sowed clean seed, free from any suspicion of contamination, on this dust. The seed of the 3 first received no treatment; in the 2 last rows it had been dampened with saltpeter solution and dusted with lime.

A scholar has conceived the idea that the dust contents of bunted grains of wheat might consist of an innumerable multitude of eggs produced by insects, whence might emerge other tiny animals capable of perpetuating the disease of which their parents were supposed to have been the cause. It was necessary either to give this conjecture some support or disprove it absolutely. It was still of interest to observe whether the dust in question, if subjected to a lively heat would lose its contagiousness. I subjected it to different degrees of heat. I took a little silver vessel, nearly the shape of a goblet, and filled it with this dust. I put the vessel in a sand bath, which I slowly heated. I plunged a mercury thermometer into the dust and removed the little vessel when it had acquired a heat of 20 degrees.

The vessel was refilled with dust, placed in the sand bath, and withdrawn when the thermometer, plunged into the dust as in the first instance, showed a temperature of 40 degrees.

Some new dust, being put into the little dish, was heated a long time, notwithstanding the fact that the surrounding sand attained an ultimate temperature of 60 degrees.

It was much more difficult for me to raise the heat to the point of boiling water. After having increased the fire under the sand and waited almost a half hour to attain this end I was obliged to abandon the sand bath as a means of heating the bunt dust. A matter worthy of note is that the thermometer, plunged into the sand, indicated the boiling point, while the dust surrounded by this same sand and which naturally should have been as hot, was not, and remained constantly at some degrees lower than the boiling point for water. I therefore tried another method to attain my object. I placed the little dish by itself in a copper pan; I put the pan on a brisk fire and brought near the vessel some burning coals. Finally, the thermometer, the bulb of which was always buried in the dust, indicated the desired

degree of heat. But it cost me a part of the dust. The middle of the mass of dust could be brought to the boiling point but not without burning that portion nearest the sides of the container and reducing the burned dust to ashes. Fortunately, the portion of the contents within the dish remained unchanged.

I blackened some choice wheat with the dust that had sustained these different degrees of heat. That that had been subjected to 20 degrees only was used to inoculate the seed of the first rows of plot 75. That subjected to 40 degrees was used as inoculum for the 3 last rows.

The six rows of plot 76 were sown, part to seed inoculated with dust heated to 60 degrees, and part to that inoculated with dust heated to the boiling point.

The 18 succeeding plots were not sown until March, 1754. I must, however, follow the thread of my discourse and relate the details of what the plots contained.

In the 77th plot I sowed darnel seed blackened with dust from bunted wheat grains.

In the 78th I sowed clean spring wheat that had been washed in ordinary well water, somewhat warmed and poured over the grain.

In the 79th plot was sown spring wheat inoculated since the 12 of January, 1754, with the dust of bunted grains of wheat.

In the 80th I sowed some spring wheat, at first dusted with smut, like that of the preceding plot, then simply washed it carefully in well water: it will be necessary always to assume that it was washed, after it had lain a long time in the contagious dust, as a preliminary to its receiving the different treatments to which I subjected it. They are here presented in the exact order in which I used them and relatively per plot occupied by the seed on which treatment was employed. Spring wheat was employed in every instance. I shall abridge my account by citing only the plot and each treatment or preparation.

Plot 81, an aqueous solution of saltpeter and lime: The lime will be present in all subsequent treatments except those for plots 90, 93, and 94.

Plot 82, an aqueous solution of common salt.

Plot 83, a mixture of 1 part of gray potash solution and 1 part of ordinary water. The basis of this solution, as I

have already said, was a pound of potash and 2 pints of water reduced to three *demi-setiers* after having boiled a half hour and been filtered through gray paper. I had noticed, as I shall soon have occasion to observe, that the saltwort and pearl-ash leaches, reduced to 3 *demi-setiers* per pound of ashes, were too strong for the seed and affected the germ; I nevertheless did not wish to omit from the number of my treatments these caustic and penetrating agents; I took part of them for tempering their effect; I mixed water in different proportions with these solutions and sought the point where their action agreed with the delicacy of the seed.

Plot 84, a mixture of 1 part of water with the same gray potash solution and 3 parts of ordinary water.

Plot 85, a mixture of 1 part of the leachings from saltwort ashes and 2 parts of ordinary water.

Plot 86, a mixture of 1 part of the leachings from saltwort ashes and 4 parts of ordinary water.

Plot 87, 1 part of the leach from pearl ashes and 2 of ordinary water.

Plot 88, 1 part of the leach from pearl ashes and 4 parts of ordinary water.

Plot 89, 1 part of the leach from ashes of new wood and 1 of ordinary water.

Plot 90, a mixture of 1 part spirits of nitre and 9 parts of water.

Plot 91, a lotion consisting of putrefied human urine, applied warm.

Plot 92, a lotion of common wash water, applied warm.

The two succeeding plots were not in the range of those under consideration. They were a little isolated, although wholly within the same terrain. Some spring wheat, similar to that of the preceding plots, was sown after it had been given the following treatments:

Plot 93, a mixture of 1 part of white potash solution with 2 of water.

Plot 94, 1 part of white potash solution and 4 parts of water.

It may be recalled that plots 37, 38, 39, and 40 of 1752 seed were excellent, well filled, and, so far as one could observe, showed only a trace of disease, as a result of the remedies applied with such striking success to the bunt-

contaminated seed consigned to these plots. Before they were reaped, I, myself, collected 3 or 4 bouquets of very choice heads. They gave me as plump and sound seed as I could desire. It was employed as such and without preparation alongside the first series of my plots in this border two feet wide and extending the entire length of the field I had devoted to this purpose.

The experiments, which I increased in number and scope and without confining myself to ordinary practice, were carried on in a tract of ground abutting on that occupied by my plots and separated from it only by a mere road. The seed employed in the one was almost the same as that sown in the other. That concerning which there was any question came from the vineyard in 1752; it was uprooted at the beginning of the following year. The owner of the piece sowed it to barley and immediately after the harvest the field was turned over to me.

Hardly had I taken possession before I had it plowed and gave it its first working. It was given a second during the course of September; a third some time after; and a fourth, toward the end of October, was the one that made it ready for seeding.

This piece of ground seemed to me good enough and, having been manured for a long time to keep up the vigor of the young vines grown there, I applied no sort of fertilizer to the soil. I had the satisfaction of finding the manure that had been applied thoroughly rotted and I was, therefore, free from exposing myself to the use of any suspected materials. It was situated between some vines, but very well exposed: 421 feet long by 38 feet wide. *When it was worked the third time, I divided it lengthwise into 2 equal parts. With a plow a furrow was made from one end to the other and this formed a sort of alleyway between the 2 halves of the field and as a guide to prevent mixing of the different kinds of seed consigned to them.

* Figure 1. — — — — —

Each half of the ground was subdivided into 5 portions. The 1st and the 5th were 60 feet long.

The 2nd and the 4th were 40 feet long, and the 3rd was 221 long by 19 wide.

In the 1st, the 3rd, and the 5th divisions of one of the

* Figure 1.

halves, which will be designated by *A*, I sowed clean, non-treated seed. I had selected it from several bundles that had been furnished me by a farmer whose seed bore some reputation. This wheat was exceedingly choice, but mixed with it were still some bunt balls that had escaped the flail.

I employed this same wheat in the 2nd division after having blackened it with the dust of bunt balls.

I moistened with saltpeter solution and dusted with lime a portion of the inoculated seed and sowed it in the 4th division.

For section *A* of this tract I had chosen, as will be seen, the best seed as much to sow it in this condition as to use it inoculated with smut or treated after inoculation. As for the wheat designed for the other part of the same tract, which will be designated *B*, my purpose, on the contrary, was to seek out the most infected seed from the bundles and without having to inoculate it myself, sow a part of it in this condition and apply to the rest of it some remedies that would protect it against injury.

The harvest of 1753 had produced for me too many bunt-infested sheaves; so many, indeed, that it was not easy for me to find wheat from the flail, or even from the fan that was not still covered with the black dust of diseased grain.

What I employed in part *B* will be understood when I say that the workman who threshed the infected sheaves where I gathered them, and who fanned them, saw the black dust from the sheaves rise like a dense cloud during the action of the fan, so that he was himself covered and frequently forced to cough, so much was he affected. I made use of this grain, thus smutted, and without any previous treatment, in the second division of part *B*.

For the 1st, 3rd, and 5th divisions the seed was prepared as follows: I put it in a tubful of water and washed it. I poured off this first water, which looked like ink. I poured some clean water into the tub; I stirred the grain and poured the water off a second time. I repeated this same operation as many times as it seemed necessary in order that the grain might be clean and of excellent color and that the water might run somewhat clear from the tub. The seed thus washed was exposed to the sun that it might dry. Later, I washed it in ordinary lukewarm wash water and

put it out to dry a second time. Then I dampened it with 3 pints of water containing a pound of lixiviated potash: the same quantity of similar potash had given 8 ounces of regular salt. I dusted the seed, still moist, with quick lime, stirring it well at the same time in order that the lime might reach every part of each seed; and when it was dry I sowed it in the 3 divisions already referred to.

The quantity of seed I thus treated could go into a bushel measure and weighed 34 or 35 lbs. The three pints of wash water was more than enough to moisten it.

In the 4th division I employed the same kind of inoculated seed, but I first freed it of that which rendered it dangerous. I first washed it in ordinary water and then in common wash water; I dampened it then with an aqueous solution of saltpeter and sprinkled it with lime.

The day on which I sowed this little patch of ground I, myself, supervised the planting of the wheat in each division and saw to it that it was sown exactly in the order I had arranged. I saw, in the first place, that all the wheat, clean as well as treated, was sown; and then that that had been dusted with smut. It was necessary, in distributing the wheat, which I tried to guarantee against all contagion, that the hand of the sower be clean and that it be free from the infectious dust of the bunted kernels.

When the ground was covered with seed, and a moment before the plow started, I set stakes at each end of the furrows that had been traced so as to clearly mark out the divisions that had helped the sower to avoid throwing seed beyond its prescribed bounds.

After the soil had been worked down, a cord was extended from stake to stake: the furrows, which the plow had filled up, were retraced and thereby I saw remade the same plot of ground I had originally laid out.

I have dwelt somewhat fully on the preparatory steps for this experiment because it fully came up to my expectations and has seemed to me worthy of attention.

I made it conform to the ordinary method of cultivating the soil. As a consequence, it will impress the farmer who, yielding but little to reason, desires the obvious things, the customary things, the facts clearly analogous to those that pass perpetually before his eyes.

If I had limited myself to a procedure of certain pre-

cautions and satisfied the eye by the symmetry and regularity characteristic of my plots, it would have been thought by some farmers that the success of this project was due principally to my care; and that, had it been conducted in the open country, it perhaps would not have been followed by such happy results as I reported.

This reasoning, easily comprehensible though it may be, will not be accepted even today.

I took for my large-scale experiment the first piece of ground at hand. I left it much as I found it, so far as concerned the fertilizers that had been applied to it; I plowed it; I sowed the seed according to custom and at the same rate as the farmers sow theirs. Thus far my practices and theirs agreed: most of our operations were the same: if there was any difference in our procedure it was only in relation to the preparation of the seed.

All of the farmers were then forced to agree that a man, in copying them, that one of their own number, if you please, proved that the dust of the kernels of diseased wheat was the true source of wheat smut and that to protect them against this disease there are certain remedies.

I feel that the reader is fatigued with having seen up to this point only a recital of a large number of experiments and that he must be awaiting impatiently the results obtained from them.

I shall present them by adhering to the order I followed from the beginning and by going back to the first sowing I made.

The details pertaining to the home experiments will follow immediately after the results from the piece of ground worked with the plow, and they will give added certitude to the interesting facts observed.

We are already informed that the little parcel of ground where, in September, I had put in 14 rows of wheat and 2 rows of darnel grass did not turn out as I had expected. I had fancied that in sowing some of the smut-contaminated seed at intervals of 7 or 8 days I would be able to detect different shades of color, even in the very beginnings of the disease in the wheat, and to follow the progress of the contagion, up to a certain point. I was deceived. The last row, sown to blackened seed, seemed to me like the second, where, also, the seed had been inoculated: It was plain that

I could consider the plants only from the standpoint of their more or less languishing condition.

I could not perceive the progress of the disease as I had hoped to do. There was an easily detectable difference between the healthy and infected wheat plants when they emerged and put out their first leaves, and especially when they were far-advanced; but, among the infected plants, themselves, the degrees of frailty were not perceptible, at least not so to me.

The first 6 rows of this little piece were perfectly beautiful before winter set in. The wheat plants were very vigorous; some of them had thrown out 4 or 5 tillers and resembled plants of rye: but the rigorous cold to which they were subject injured them not a little. A great part of them perished; the plants that were marked to bear smut heads nearly all died. I was scarcely able to find one of these latter in the rows that had been sown to bunted seed. Moreover, the other rows, the darnel as well as wheat, were in ordinary condition and produced about what I expected, that is to say, the majority of heads bunted wherever the seed had been inoculated and were very healthy in those plants grown from clean, carefully selected seed.

From the last observation I made it would appear (1) that there is some wisdom in not sowing wheat too early, at least in certain climates; for the severe frosts are capable of killing plants that have much vigor and whose stems already show evident growth. On the other hand, plants that have hardly more than emerged resist these freezes and escape, underground, from any harmful effect from external cold, which the tillers of too-far-advanced plants apparently cannot withstand.

(2) That the bunt-infected plants are more apt to die during rigorous winters than are healthy, well-formed plants. These people believe that, in some way, severe remedies weaken the plants in which a baleful leaven still resides, and, as a consequence, a serious disease nearly always proves fatal.

I shall not dwell on this striking alternative of healthy and bunted heads, yielded by the ten rows of the little piece as having any relation to the seed sown there. This and the certainty of a virus in the dust of the bunted kernels of wheat, as well as darnel, and the very real advantage of

sowing only sound seed have been demonstrated beyond question.

From the condition of the first 20 plots, where manures of different kinds were applied, it has lately been decided that suspected straw, mixed with the excrement of animals, contained no longer anything dangerous to the seed.

It has not been overlooked that each kind of fertilizer had been applied to 4 plots of which the first two always contained clean, noncontaminated straw and the other two, suspected straw. There was not a bunted head in plots 1, 2, 3, and 4, where cow manure had been applied. I saw only one in the 3rd of the 4 plots where sheep manure had been used; I saw none at all in the remaining three. There was none in the 13th, 14th, 15th, and 16th plots into which dry sheep manure had been worked; none, in fact, in the 4 following plots where I had employed sheep manure.

The 5th, 6th, 7th, and 8th plots contained horse manure. I noticed 6 bunted heads in the first two and 10 in the other two. Since the plots into which clean straw had been worked bore 2 or 3 diseased plants, it is necessary to refer back to the seed to find the source of the slight infection and to assume that if bunt is manifest in some heads in the two plots containing suspected straw this is not a result to be attributed to these lots of straw but rather to some contaminated seed mixed in with the seed sown in these plots.

It has been noted in the description of the experiments of 1752-53 that the 19th and 20th plots produced a great many bunted heads. I attributed the cause to suspected straw, which did not lose its contagious principle when worked into the two plots containing sheep manure.

As we shall see, this was not the case in 1753-54: Not a diseased head could be found in the 15th and 16th plots, although they contained as much sheep manure and suspected straw. It is true that the dry manure of which I made use the second time was not precisely of the same quality as that employed in the first instance. A great quantity of hay did not underlie it as it did in the other lot.

Being unable to find some that was absolutely like it, I used that which differed the least. The latter was, even at best, only the dried droppings of the animal: without doubt it served the purpose of the dung of other animals in relation to the suspected straw and was an excellent detergent.

There were in these 20 plots some heads of smut [loose smut], which were not traceable to one sort of fertilizer rather than another nor to suspected rather than clean straw.

Four or 5 heads affected with ergot were found, as many in the first three rows of plot 21, sown with clean rye, as in the last 3 in which seed previously contaminated with the dust of powdered ergot had been sown: further proof of the fact that this dust carries no contagion for the seed.

In spite of the cold weather (approximately 15° below zero, Reaumur) of February 2, 1754, I saved several of the little grubs, or, to better express myself, the little caterpillars I had found in some of the rye ergot grains on which they had fed and concerning which I have already spoken. Four of them metamorphosed into quite pretty butterflies, the wings, legs, and antennae of which were studded with white spots. Others were similarly covered with dark musk spots.

These butterflies were of the smallest sort and seemed to me to be quite common. I have seen their like, or I am much deceived, on the surface of water, exposed to the sun, in a washtub—water to be used later for garden irrigation.

I do not recall ever having seen them in the open country: it is none the less true, however, that butterflies of this kind may have oviposited and pupated there, for I am wholly ignorant of the special technique by which they protect their offspring and prepare food for them in the very retreat within which they develop. It is none the less true, I say, that they may have attached their eggs to some grains of rye out of which came the little caterpillars that I raised; that these grains, changed into ergots through some kind of derangement in their structure, served as food for the caterpillars; that the latter metamorphosed into butterflies; and that these insects became in turn the cause of several ergots in laboring for the conservation of their progeny.

I have every reason to believe that the ergot commences to form through an oozing of the juice contained in the kernel altered by the insect. One of the plots, to be considered when I report on the home experiments, was darnel grass. In examining it one day, I noticed a kernel of this kind that was much larger than the others and partly covered with a honey-like liquid. I marked the head containing

it and observed it from time to time. The unctuous liquid which adhered to it, gradually dried, the kernel enlarged, elongated a little, became black, and changed into a real ergot.

Either a large number of rye kernels, merely altered by the insect, become converted into ergots, contain no caterpillars because of absence of eggs or a large number of eggs perish because of divers untoward circumstances. I gathered only the small lot of ergots that harbored the caterpillars which I reared after having rejected many others in which there appeared to be no vestige of an insect. And, this year, out of 200 ergots, or thereabouts, I found but four that contained caterpillars. They made themselves known in a rather striking manner. The end of the ergot enclosed by the glumes and where, without doubt, the egg hatches is ordinarily pulpy, slightly moist, and apt to become spoiled. A little hole could be seen there that is the entryway of a sort of gallery that runs the length of the ergot and that the insect made in the process of feeding itself. One nearly always sees on the epidermis of the ergot a tiny opening that communicates with the gallery and by which the caterpillar rejects a portion of the material it devours. This residue may be compared to the fine dust one observes on wood tunneled by worms in the little channels where the insects are lodged.

Sometimes a caterpillar entirely consumes the ergot in which it is concealed leaving the outer covering reduced to dust. Last summer I put an ergoted grain of wheat in a small glass goblet and my intention was solely to keep it there because it is very rarely one finds ergot in wheat. This ergot, by chance, contained a caterpillar. It had lived there; the ergot was entirely consumed; the rind reduced to dust, conceals the insect today and there it will remain, no doubt, until time for its metamorphosis.

It is observed that there are years when ergot is very common, others when there is very little, and that the rye of certain countries is more subject to this disease than is that of others. The optimum time for the propagation of the insects that cause it, the divers circumstances that may cause their death, the great number of these little animals, once spread abroad in a country and proportionately multiplying there, seems to me to be a natural explanation of

these differences; as, in the case of another sort of insect, they clearly explain why our fruit trees are sometimes severely damaged, sometimes quite free from damage; and why, in some cantons, a reasonable quantity of fruit is rarely harvested.

The dust produced by the grains of bunted wheat certainly possesses no contagious principle for rye. Some rye was bunt-dusted before being sown in plot 22; not a head, however, gave the least indication of bunt.

This same dust has no virulence whatever for 4-row barley. The 3 last rows of plot 23 and all 6 rows of the 24th, the seed for which had been smutted bore heads as sound and healthy as those of the 3 first rows of the 23rd plot in which clean, bunt-free seed was sown.

What is more, 4-row barley, according to my observation, seemed subject neither to bunt nor smut nor ergot. For several years I noticed no trace of these 3 diseases on 4-row barley. Rust sometimes attacks it.

The 8 plots devoted to variously treated darnel-grass seed never threw any light on the efficacy of the remedies I employed for their protection against bunt. The severe freezes killed more than three-fourths of the plants and the loss was especially marked among the diseased plants. We have several times observed that the dust of bunted seeds of darnel was fatal to darnel. The seed of the three last rows of plots 25 and 26 had been dusted with this kind of smut and should have produced a large number of diseased heads.

Nevertheless, in the number of plants yielded by those 6 rows I was able to find scarcely one that bore a diseased head.

One may easily conceive that, for the time, the 3 rows of the same 2 plots, where I had sown clean darnel seed, where the seed had been subjected to different treatments, there must have been no bunt. I searched for it without success.

From the condition of the plots sown with treated seed one can draw no conclusion as to whether or not the remedies used were effective, for the diseased plants, if there were any, had winter-killed and there was nothing left for examination.

One may, it is true, presume from the advantageous effect which the same remedies produce on wheat that they

were favorable for the darnel and prevented the infective dust with which the seed had been contaminated from becoming a principle of contagion. But I wish to state as a certainty only that that is revealed in the light of repeated experiments and has become decisive through satisfying the objective for which they were designed.

There comes to mind here a matter for consideration. It has been observed that in certain years the darnel is very abundant among the rye plants and that sometimes very little can be found. It is known that this grass very largely succumbs to the rigors of winter. It would be quite natural to believe that where darnel is common everything has favored its growth and that, on the contrary, it has been overtaken by untoward circumstances when it is scarce. This idea would appear reasonable and ought not to offend one's good sense, like that of certain husbandmen who believe that rye sometimes changes to darnel and that there are years when this transmutation is more favored than in others by the weather conditions. I shall not give serious consideration to this grotesque idea. It will suffice here to guarantee to the farmers that they harvest only rye to the end that the seed they use be free from darnel and to assure them that the abundance of this weed is a result of their carelessness. I tell them this, and I repeat it, as an absolute fact, that I have never harvested darnel at any time when I have sown nothing but pure rye; and that I have never found any rye where and when I have sown only darnel.

In the eight plots under consideration the darnel plants that had survived the cold were few in number, but strong and vigorous. They had thrown out several culms whence developed heads that were long and laden with grain. The rows were much better filled than I had dared to hope for, so much had these eight plots at first seemed destitute of darnel plants. It is true that in the spring I had taken the precaution to eradicate all the weeds that had grown in abundance among the darnel plants and I took particular care to see that the few remaining ones were in no wise damaged by the vigorous vegetative growth of the large number of plants I had lost.

This, again, is proof of the advantages that result from the new method of cultivating the soil. A few seeds well distributed develop without injuring each other; the plants

can easily extend (have room in which to grow); the air impinging on all sides strengthens them; and the heads, abundantly provided for, yield a large quantity of seed.

There perhaps had been some hesitancy in believing that the suspected straw was dangerous for the seed, seeing that nearly all the first plots, where this straw had been employed as part of the manure, contained not a single diseased head and there were only a few where any were found. But, in addition to what has already been said, that the suspected straw is harmful to seed in its near vicinity and that one cannot use it with safety in so much as it is mixed with the dung of animals; it will be necessary in any event to yield on this point when I shall assert that plots 34, 35, and 36 where, it will be recalled, I had placed chopped contaminated straw, spreading it in the furrows, and mixing with it a little of the soil in order to facilitate germination of the seed; when, I say, I shall assert that those 3 plots contained many bunted heads. It is impossible to go so far back as the seed sown on this straw in order to find the cause of the trouble; this seed was the same as that I used in plot 33, into which had been worked some healthy straw and wherein I noticed 3 diseased heads; it was the same as that sown in plots 39 and 40, where I observed not a single bunted head. It is true that the disease was more pronounced in plots 34 and 35 than in plot 36; but it also is true that the contaminated straw, spread in this latter plot, was absolutely free from heads. I had, however, left them in the straw used in the two other plots. And here is new proof that the contaminated straw had something pestilential about it, either that it actually was so or that it had every appearance of being so, since it acquired a malignity only through accidental contamination with bunt dust when the bundles were threshed. For, in removing from it the bunted heads I eliminated from this straw the principal source of its banefulness and rendered it less injurious to the grain.

I am now certain that miracle wheat is subject to bunt, but decidedly less so than other varieties. I found only a few bunted heads in plots 37 and 38 and in the dozen rows of this wheat that I had sown separately. The seed sown in these two plots and in another isolated spot had been dusted but with dust from the bunt balls of ordinary wheat, and I

had neglected nothing in an effort to insure firm adhesion. Plot 37, as well as 38, was sown on the 24th of October. I do not know why the miracle wheat failed to come up. I presume it was too deeply planted; in fact, I recall that this plot was a little lower than the others; that the soil had unwittingly been removed from it in order to reduce it to the level of the neighboring plots when the wheat was sown, and that thereby a stratum of soil was uncovered that was too stiff and through which the wheat plants were unable to force their way. I reseeded this plot 37 in the month of March, using miracle wheat infested with bunt, like that sown in the first instance. It came up nicely, threw out a number of fine tillers, and soon attained the height of the wheat in the neighboring plot. This experiment seemed to promise that, given a good seed bed and everything else favorable, one may successfully sow miracle wheat in March and have it come to maturity.

I have already stated that plots 39 and 40 contained not a single head of bunt. The seed to which they were sown had come from choice heads, I, myself, had gathered from standing plants at the time of harvest. In one of these plots clean seed had been sown and without any previous treatment. In the other the seed had been washed in a potash solution and then dusted with lime. The perfectly healthy condition of these two plots demonstrated clearly that when the seed is clean every principle of the disease is banished, and that treatments would be superfluous if it were possible to obtain seed that had not come in touch with anything contagious. But, unfortunately, in a great many very beautiful heads that had been gathered with care there was sometimes found a diseased kernel—a partly sound, partly bunted one. This kernel escaped detection; mixed with the others, it contaminated them, soiled them with bunt dust; or if one part only of this seed is blemished, the germ not being at all affected, it is the progenitor of a diseased plant, it transmits the virus more decisively than it had, itself, received it and passes it on in full to its posterity. This reflection, which it is but natural to make here, will have its application when I speak of plot 69.

Let us recall for a moment what has been said, in the general exposition of my sowings, on the subject of the kind of seed sown in plot 41. The bundles whence I had col-

lected this seed were in great part infected. It was, itself, covered with dust from the bunt balls and was intermixed with many other and diseased grains, which the flail had not crushed. It was from this small lot of wheat, so capable of perpetuating infection, that I first drew a portion, such as it was, for plot 41. It should here be mentioned what would have transpired in the other 23 plots, if the seed sown in them, and which I drew from the little pile of infected grain, had not been insured against contagion by excellent preservatives. Of the 2,700 to 2,800[2] heads in plot 41, at least a third of them showed bunt. Such were the very marked results of the contamination of the seed sown in this plot. Such they were, moreover, wherever this seed was sown without disinfection; such, in fact, is the sort of evidence we must keep before us in going over the plots designed to show the effect of remedies if we are successfully to decide their efficacy.

I was satisfied simply to wash, but carefully, in ordinary water the same infested seed I had sown in plot 42. This precautionary measure was not sufficient to completely eliminate the cause of disease.

I noticed some diseased heads in this plot. Aside from this it was in very good condition. The mere washing in ordinary water was consistently given the seed sown in the 22 following plots before subjecting it to the various treatments.

One of the better methods of treatment, and one that causes no injury, is to wash the seed in ordinary wash water. Only one bunty head was found in plot 43.

Lime water, almost boiling hot, poured over the seed sown in plot 44, had a marked effect and, in general, should be considered advantageous.

The seed sown in plot 45 had been washed in ordinary wash water and dusted with lime. Nevertheless, there were 6 bunted heads. We shall see no more of them in the 11 plots I am about to refer to; I did not discover a single diseased plant in them, so completely successful was the remedy. It was due to the lye from new-wood ashes in plot 46. It has not been forgotten that the seed was dusted

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[2] It will be necessary to assume nearly the same number of heads in each plot; as many sound as diseased.

with lime immediately after it had been dampened by the water of the different washes.

In plots 47 and 48 success was due to the lye from pearl ashes; that of plots 49 and 50, to the lye from saltwort; that of plots 51 and 52 to potash solution; that of 53 and 54 to a saturated solution of sea salt; and that of 55 and 56 to a saturated solution of saltpeter. There is a very real satisfaction in comparing with these 11 plots the 41st plot; to see the one for the most part full of diseased heads, and the others containing nothing but sound heads; and to recall that for the 12 plots the seed of each had been taken from the same infested lot, and to realize that the several treatments were alone responsible for the difference. The success realized through the application of some other remedies was nearly as complete as that just referred to.

The putrefied cow urine, employed as a treatment of the seed sown in plot 57, reduced the amount of bunt to a single head. With the same sort of urine, combined with powdered quick lime, there were but 2 heads of bunt in the 58th plot. In the 59th plot, the seed of which was treated with putrefied human urine, there were only 3 bunted heads. The 60th plot, treated with the same kind of urine, combined with powdered quick lime, produced but 9 bunted heads. The 61st plot, the seed of which was treated with liquid manure, contained only 3 bunted heads. Plot 62, treated with liquid manure combined with powdered quick lime produced but 2 bunted heads. With a mixture of spirits of nitre and river water in the ratio of one to nine, plot 63 yielded only one diseased head. Plot 64 was sown to seed treated with the following mixture: ⅛ spirits of nitre, ⅞ river water, and pulverized lime. But six heads of bunt were found in this plot.

From the foregoing it is obvious that the treatment involving the application of powdered quick lime to seed dampened with putrefied human urine was not quite so effective as was the application of urine alone. Let us note again that spirits of nitre without the addition of lime proved more effective, even though it was weakened by the added water, than did the mixture employed in treating the seed of plot 64. This alkaline substance probably too quickly combined with the spirits of nitre and before it had had time to produce its prophylactic effect.

The seed used in sowing the 4 succeeding plots was from the 1752 crop and came from the same bag. In plot 65 I sowed clean, nontreated seed. It produced not a single bunted head.

There was every indication that the wheat that had been left covered with bunt dust for a year acquired no more susceptibility to the disease and became no better adapted to spreading the contagion than if it had been covered with bunt a few days before seeding. Plot 66, for which I had used seed that had been buried in bunt one year, produced about a third of a crop of bunted heads. This is a commonly experienced loss and the disease is sometimes more pronounced in places where the seed has been sown soon after inoculating with smut.

In superficially washing this infested seed, in dampening it afterward with potash solution, and dusting it with lime, I removed from it nearly all of the contagious principle. I found only 3 heads of bunt in the 67th plot.

The saturated solution of saltpeter with which I moistened the seed for plot 68, combined with the quick lime, which I applied immediately after, proved a sovereign remedy. Plot 68, where this seed was sown, contained not a single bunted head; at any rate, I noticed none there.

Such, again, is the spectacle we have several times delighted in and which is always agreeable to observe. There, some clean, carefully selected seed which yields none but healthy heads. Here, the same seed, infested with bunt, is a well-spring of diseased heads; whilst, on the other hand, this same seed loses through a treatment that which makes it injurious and causes it to produce only sound, healthy heads.

In plot 69 some bunted heads were found, although the seed sown in that plot was apparently very clean and had been selected from heads I had carefully collected.

Now is the occasion to apply what has been stated in the foregoing. A seed affected with bunt may be encountered in a head otherwise sound enough to escape detection of disease. This bunted kernel might be crushed in threshing the head and become the source of disease in good seed smeared with the infective dust. A partly smutted, partly healthy, kernel has been found in a head of excellent appearance and has slipped into the seed used for sowing only to become the source of a diseased plant.

The same élite wheat had been contaminated with the dust of bunted darnel seed and sown in this condition in the 70th plot. Two-thirds of the heads produced were bunted. The experiment repeated showed again that the dust of bunted grains of darnel is at least as dangerous for wheat as is that of its own bunt.

The bunt disappeared entirely in the 71st plot. Every plant without exception was healthy. Potash solution and powdered lime had removed from the seed sown in this plot the external principle of the virus. Without this treatment, it would have been the source of a disease similar to that we observed in plot 70, since, before receiving the treatment, it had been inoculated with the dust of bunted darnel seeds.

The saturated solution of saltpeter, without question, is one of the best preservatives I have noted. Not a single diseased head could be found in plot 72. The dust from the diseased darnel kernels was, in spite of its pathogenicity, without effect on the wheat sown in that plot.

There yet remains for us to give special consideration to the different and very marked results obtained, not only from the standpoint of the soil, but also with respect to the condition of the seed employed. Sometimes, used in all its purity, it produces nothing but sound heads; at other times, covered with an apparently innocuous dust, it becomes an abundant source of disease; or, in this latter condition, which may result disastrously, it is the cause of no trouble whatever because it has been subjected to a simple and apparently superficial treatment that protected it against the disease principle.

That trail of smut, which I laid in each furrow of plot 73, half-an-inch from the seed, proved as disastrous as it did in 1753. At least a third of the heads produced in this plot were found bunted. This seed was pure, however, and carefully selected. Employed in this condition of purity, it had yielded in several places none but sound, healthy heads. Moreover, the precautions I observed will be recalled: The care I took to prevent contact between the seed and the trail of smut and to prevent the young plants from touching it until their first roots came in contact with it.

Is it not natural to fancy these same roots, still delicate and susceptible to the slightest impression, contracting the contagious poison of the smut through the development of

their rootlets in the path of the smut? Is it not natural to picture these rootlets as having been the initial avenue of the virus? Is it not natural to imagine that they transmitted it to the nascent stem and thence, subsequently, to the embryos enclosed within the head?

If a trail of smut, neatly laid down at some distance from the seed, was harmful to it, it is not surprising that this same smut, spread in the furrows of plot 74, where I sowed some seed, should have been dangerous and should have infected that seed before it had developed any plants. The majority of the heads in this plot were bunted and were indistinctly visible in all the rows. I made this latter observation because the seed I used for the first 3 rows was clean, though nontreated. That of the last two I treated with a saturated solution of saltpeter and then dusted it with lime before sowing. This would seem to prove that if, in the last two rows, half of the heads were diseased in spite of the seed treatment, it was because the infective principle had reached the young roots in their ramifying across the smut spread in the furrow bottoms and sufficiently abundant to cause a general infection.

The different degrees of heat to which I had subjected the bunt dust employed in smutting the seed sown in plots 75 and 76 rendered it only the more active in infecting the seed and to a degree quite proportionate to the degree of heat employed. Three-fourths of the heads in plot 76 were bunted, where, I stated, one part of the seed sown had been smutted with bunt dust that had been subjected to a temperature of 60°, and another part with the same dust that I had had so much difficulty in bringing to the boiling point. There were fewer bunted heads in plot 75. The dust with which I inoculated the seed sown in the 3 first rows of this plot had been subjected to but 20° temperature and that used as inoculum for the seed of the 3 last rows was heated to 40°.

It is by all means necessary to abandon the hypothesis that the dust of kernels of bunted wheat be thought of in the light of an ant hill of eggs that, on hatching, produce a prodigious quantity of very small insects that one might suspect of being the originators of the malady affecting the seed. A moderate heat such as 20° or 40° would have favored the birth of the insects, if indeed there had been

any. One would have been able to see them with the aid of a magnifier, and an appreciable and very characteristic damage would have been the result of their sojourn in the wheat head. I observed nothing of the kind. A great part of plot 75 was infected with smut, it is true; but, as in the other plots where there was bunt, the disease was present in the very midst of the plant; there the cause had been insidiously active; and I had seen only the outward evidences by which a deep-seated disease becomes manifest. A temperature of 60°, or one as high as that of boiling water, having been applied to the bunt dust, would infallibly have killed the eggs of insects, which we assume here to have been nothing other than the cause of this same dust, and, consequently, the disease would not have appeared in plot 76, since, according to the hypothesis, the insects from these eggs would alone have been able to produce it. However, we have observed that this same plot was one of the most severely infected and that three-fourths of the heads were found diseased.

The plots now about to be considered were sown in the first days of March, 1754.

The 77th plot contained darnel: I discovered there only one or two diseased heads, notwithstanding the fact that I had blackened the seed sown in this plot with dust from bunted wheat grains. Several extra rows of darnel that I had sown with seed left over from this same inoculated lot, gave me nothing but healthy heads such as one might have anticipated had one taken precautions against untoward circumstances.

What caprice! The dust of the grains of bunted darnel is fatal to darnel and to wheat. The dust from bunted kernels of wheat is pathogenic for wheat and innocuous for darnel. With what moderation must one reason from one fact to another, what analogy may one not suspect between them! All indications are that the two kinds of contagious dust should act reciprocally on the two kinds of seed, since one kind of dust is capable of this double action. All the experiments I have thus far made have been contrary to appearances and the darnel has never responded to the malignity of the wheat smut. This phenomenon raised my curiosity. I shall, this year, have opportunity to observe it and I shall

not fail to render an account of it in reporting the experiments on which I am at present engaged.

It is only too consistently true that, as I have indicated in my dissertation, spring wheat is more subject to bunt than is winter wheat. The material comprising my experiments in the 17 plots, the results of which I have yet to give, yielded some sheaves that I had threshed, leaving in them the small number of diseased heads they contained. Consequently, the seed was under suspicion and called for treatment. I washed in ordinary wash water the portion of this seed set aside for the 78th plot. It, nevertheless, produced several bunted heads. This wash, the reported use of which on winter wheat we have already noted and concerning the success of which we shall yet have opportunity to observe, was not wholly effective on spring wheat.

Let us consider how susceptible spring wheat is to the bunt-dust virus: Infected with smut it becomes the source of a disease that is nearly general in plot 79. In a total of 10 heads I was able to find scarcely one healthy one.

This same wheat not only becomes prey to the pestilential principle resident in smut, but it retains tenaciously a residuum of this principle, although by repeated washes one seemingly leaves in it nothing contagious. The condition of the 80th plot evidently confirms this, for I noticed in it many diseased heads. The seed sown in this plot had been allowed to lie smutted about 2 months. I had washed it well before sowing, but a part of the seed always retained the malignant principle. It required active remedies, those whose effectiveness already is known. Let us not forget that the bunt dust always was applied to seed such as this; that is to say, first inoculated and then washed in several waters.

I observed but one bunted head in plot 81. The saturated solution of saltpeter had produced its effect. The saturated solution of common salt had been less effective; for there were 6 diseased heads in the 82nd plot.

There were 7 bunted heads in the 83rd plot and an equal number in the 84th. The remedy applied to the seed of these 2 plots was a solution of commercial potash that I had more or less diluted in the fear that it might otherwise affect the seed. Concerning this, I have already spoken.

In the 85th plot I found 11 bunted heads and an equal

number in plot 86. The remedy applied in these 2 plots was a solution of saltwort lye. In each of plots 87 and 88 I found but 2 bunted heads. The leach of pearl ashes had exerted some action in these plots. I saw only one bunted head in the 89th plot. The leach of new-wood ashes was responsible for this result.

I noticed only a trace of the disease in the 90th plot, the seed of which had been treated with a mixture comprising one part of the spirits of nitre and nine parts of water.

Putrid human urine was by no means so effective a remedy for the spring wheat sown in the 91st plot as it was for the winter wheat and as we shall yet note it to have been in the small-scale experiments. This plot was not in such good condition as the preceding ones; several plants were affected with bunt.

I presume that the quick lime, with which the seed sown in the 92nd plot was dusted on removal from the ordinary wash water, was beneficial. With the exception of a single bunted head, all those produced by this seed showed no smut; while that employed in plot 78 and that had been merely washed in the same kind of water as that applied to the seed sown in plot 92 and had not been dusted with lime, did, as we have observed, produce several diseased heads.

The 93rd and 94th plots, both isolated, fully met my expectations. The wheat plants were vigorous, the individual tillers were tall, the heads filled with grain. They resembled the finest plots of winter wheat. I had, above all, the pleasure of finding only one head of bunt. The remedy employed in treating the seed of these 2 plots was an aqueous solution of caustic potash, more or less amended, and powdered quick lime.

Let us now return to the 79th plot, in which there was almost a maximum amount of the disease, since nearly one-tenth of the heads were affected with smut. Let us examine the effect of the somewhat less effective remedies on the spring wheat, which, compared with winter wheat, was always more satisfactory. Let us not forget that the same seed served as a basis for the different tests under consideration and that it differed from plot to plot only externally. Let us now observe the 2 plots before us and we shall boldly conclude that the dust of bunted grains of wheat is very viru-

lent for spring wheat. It is the recognized source of the disease and perhaps the only one in our clime; it certainly is the source of the greatest abundance of smutted heads. This dust, whether totally removed or arrested in its action, is no longer dangerous for the seed, howsoever inoculated, when subjected to the simple remedies I have indicated and that the farmer will find at his disposal.

The rows of wheat I had put in along the first series of my plots gave me some well-formed, healthy heads and among them I found very few containing any bunt. As is known, I sowed them to clean, nontreated seed obtained from a sowing of seed the purity of which was under suspicion and to which had been applied one of the best remedies without divesting it of the smut with which it had been inoculated. The result was that I found the wheat of those rows, the inoculated seed of which had been treated, in a healthy, sound condition. It had been radically protected against bunt and in it there remained nothing of the pernicious principle to be passed on to subsequent crops. That one's success is not always so satisfactory as it should be when art comes to man's assistance in this insidious, troublesome disease which calls for active, penetrating medicaments that do not always prevent under conditions of faulty application, is sometimes made obvious by external symptoms or by the unhealthy appearance of those plants one has brought forth.

In presenting the distribution of my plots, and my different seedings, I have neglected to say that at the end of the border I am now about to consider I had reserved a little space where I put in 20 rows of wheat. These rows were sown at right angles to the long axis of the border in order to avoid confusing them with the others that had been sown lengthwise. The seed of the first 5 rows was of clean wheat; that of the succeeding 5 was from the same wheat, blackened with the dust of bunt balls, that I had held in reserve one year. The seed of the next 5 had been inoculated with bunt dust obtained from only partly developed bunt balls: the head from which I obtained them had been collected on June 18, 1753; that is to say, about 5 weeks prior to harvest, when the smutty matter within the diseased kernels was soft and greasy. It was subsequently dried and I reduced it to a dust before using it. The seed of the 5 last rows was

inoculated with the dust from well-formed bunt balls, such as one obtains from thoroughly mature plants. There was not one bunted head in the first 5 rows. There were many in the remaining 15 and almost an equal amount in each.

Let us first note, then, that pure, clean seed comes only from sound healthy heads. Second, that from contaminated seed a great many bunted heads arise. Third, that smut, kept from one year to the next loses none of its virulency. Fourth, that this same smut, although imperfectly formed, is dangerous to the seed and already possesses the virulent principle that causes the malady.

In the order of succession in which I have presented my experiments and have given the results I am inclined at the moment to recall one of the most essential and give an exact account of it.

That to which I have here referred in relation to my plots relates to something that may have been lost sight of in what I said little farther back concerning the manner in which I laid out the ploughed ground and the condition of the seed I sowed there.

Let us take a casual glance at the project before looking into the condition of the wheat produced in this tract. The length of this little field was 421 feet and the width, 38 feet. It was divided lengthwise into 2 equal parts; these I designated A and B (Fig. 1). Each of these parts was subdivided into five portions, the first and last of which were 60 feet long and 19 feet wide. The 2nd and 4th were 40 feet long and the 3rd, 221.

The wheat sown in plot A was originally clean, carefully selected, but mixed later with some bunt balls. I sowed it in this condition, and without giving it any preliminary wash, in the 1st, 3rd, and 5th divisions. I inoculated with bunt the seed for the other two divisions also, but with this marked difference, namely, that the seed sown in the 2nd division was sown without treatment, while that in the 4th I moistened with a saturated solution of saltpeter without washing off the smut and then dusted it with lime.

The seed sown in plot B was originally all that was necessary as a source of disease. It came directly from under the flail, covered with smut as much so as though I had purposely inoculated it. I sowed it in this condition in the 2nd division.

For the 1st, 3rd, and 5th divisions it was washed in ordinary well water; then in wash water, and then in potash solution, after which it was dusted with lime.

Instead of the aqueous solution of potash, I used a saturated solution of saltpeter on the seed sown in the 4th division, after having washed it in ordinary well water and then in ordinary wash water.

Some bunted heads were found in the 1st, 3rd, and 5th divisions of part A. This should not surprise us. I stated that the seed sown in these divisions was mixed with some bunt balls. Their proximity was dangerous to the sown seed. As for the 2nd division, two-thirds of the heads were found diseased; moreover, the culms were shorter. I did not need to hunt for the two furrows that constituted the limits of this parcel of ground in order to set it off precisely from the 1st and 3rd divisions. One was at once struck by the backward condition of the wheat contained in it, by the bluish color of the heads, and by the shorter culms already alluded to. From the appearance of marked superiority of the culms, that bordered the first and third divisions alongside the second, one would have believed at first glance that the culms of the second division had grown in a slight depression of the terrain and that the level had suddenly recovered in spots where the wheat stood higher.

The saturated saltpeter solution followed by lime gave marvellous results in the fourth division. Almost no smut developed: We have observed the consistent success following this treatment.

I come now to part B, which was sown to seed thoroughly capable of perpetuating the disease. The treatment applied to the seed of the 1st, 3rd, and 5th divisions consisted of dipping it in an aqueous solution of potash and dusting with lime after giving it the customary washings in ordinary water and in wash water. Bunted heads in these three divisions were rare, much more so than in the corresponding three divisions in part A. The seed I used for these three divisions must, nevertheless, be regarded as very excellent and would have been accepted as seed by the exacting farmer. There are, then, some real advantages consequent upon certain treatments applied to seed; the success of these treatments is, therefore, quite favorable, since the most bunt-infested seed loses as a consequence whatever it

carries by way of contagion and produces thereafter much less of the disease than does élite seed, very satisfactory to the eye, but mixed, nevertheless, with bunt balls and not treated before sowing.

The disease was present in the 2nd division of part B. It was not observed in so many plants as in the corresponding division of part A. The reason for this is obvious. I had purposely and copiously smutted the seed sown in this latter division. The seed I used in the 2nd division of part B, although heavily inoculated with smut, carried less inoculum than that sown in the 2nd division of part A. I used it in such condition as I found it on leaving the fan, devoid in the main of the bunt dust that covered it as a result of the operation of the flail. This dust, being the source of the disease, produced a more or less marked effect according to the more or less abundant dose of smut received by the seed. The disease, I stated, prevailed in the 2nd division of part B: The considerable number of bunted heads in this division therefore indicated that a like infection surely would have occurred in the four other divisions but for the fact that helpful remedies had been applied to the seed that I had sown in them. Since it [the seed] was originally the same as that that produced such a large number of bunted heads, this second division was, therefore, the spot whence issued the pronounced ray of light on the veritable cause of the disease: it was this unsuspected evidence, that I have always made it a rule to allow to stand, when it is desired to test a remedy on some bunt-infested seed and remove from it every element of contagion.

There was scarcely a vestige of the disease in the 4th division of part B. The washings with ordinary [well] water and with wash water, which preceded the treatment with a saturated solution of saltpeter, followed by dusting with lime, seemed superfluous. The saltpeter solution, plus the lime, was in itself a sufficient preventive for the seed I sowed in the 4th division of part A. The bunted heads were quite as rare as they were in the corresponding division of part B, where this same treatment had been applied only after the seed had been given the two preliminary washings referred to in the foregoing. Nevertheless, these preliminary baths would no doubt be very useful in instances where a less active preventive agent than nitre solution and lime,

or some other that I have seen work with complete success, might be employed.

The large-scale experiments of which I am speaking carry conviction on the fundamental points that I have made my objective. Let us, however, compare with these experiments those I made on a small scale and which I followed each day because they were daily before me. If pictured in the ensemble they will together confirm what I have asserted, and the least reasonable doubt will no longer be entertained.

The little garden surrounded by buildings, where, in 1751 and 1752, the experiments were conducted of which I have already given the details, was used again in 1753. It will be recalled that it comprised two squares of unequal size, the dimensions of which have already been given.

I made, as in the preceding year, two plots in the first and four in the second square. Each of the plots of the first square contained six rows of wheat; there were but four in each plot of the second square. The wheat I sowed in each of the six plots, if we except the two last rows of the second square, where it was necessary to sow some clean seed on some prepared manure; this wheat, I say, was originally contaminated with smut and was the same as that that comprised the material for plot 41 and 23 others, the results of which I am here presenting.

The first row of Plot A (Fig. 2) of the first square was sown to this bunt-infested wheat; the 2nd row to some of the same seed merely washed in ordinary water. It will be necessary always to assume the seed to have been thus washed before treating it with the remedies that follow. The 3rd row was sown to the same seed dampened with ordinary wash water; the 4th, the same, moistened with potash solution; the 5th, the same, wetted with a saturated solution of common salt; and the 6th was sown to the same seed, moistened with a solution of saltpeter. The seed sown in the 4th, 5th, and 6th rows was dusted with lime after each treatment.

The first row of Plot B was sown to the same bunt-infested seed. The treatment materials employed on the seed for the five remaining rows were putrefied bovine urine for the 2nd; putrefied human urine for the 3rd; a mixture of one part of spirits of nitre to seven parts of water for the

4th; a solution of lye from the ashes of green wood for the 5th; and a solution of lye of pearl-ashes for the 6th. This seed also was dusted with lime following the three last treatments.

The first row of Plot A (Fig. 3) of the second square was sown to the same bunt-infested wheat. It was simply washed for the 2nd row. The remedies applied to the other two were common wash water and a mixture of one part of spirits of nitre and seven parts of water.

The remedies applied to the seed sown in Plot B were an aqueous solution of soda for the 1st row; lye of pearl-ashes for the 2nd; a saturated solution of common salt for the 3rd; and a saturated solution of saltpeter for the 4th.

The seed employed in the last row of Plot A and in the two first rows of Plot B was dusted with lime before sowing.

Plot C contained nothing but darnel grass. The seed of the first row was clean; inoculated with the dust of darnel smut for the 2nd; moistened in this condition with soda solution and dusted with lime for the 3rd; and dampened with saturated solution of saltpeter and dusted with lime for the 4th.

The wheat sown in row 1 of Plot D was washed with putrefied human urine; that of the 2nd row, with liquid sheep manure. That sown in the two last rows was clean: I sowed it on dry sheep manure combined either with clean or with suspected straw.

I shall not enter upon the details of the product of these two squares. One can see exactly what happened by examining the diagrams I have included at the end of this supplement and in the conclusion of which are the results of each plot. The crop I obtained from the home experiments was the result of my own work; nobody was with me while at work. According as I pulled up the wheat plants of a row I separated the sound heads from the bunted ones. I faithfully counted those of each lot and made a note of the result in referring them back to the particular rows from which they had come. If one cares to depend upon my accuracy and accept the results I have furnished, he will have the satisfaction of seeing how that same seed which, in the 1st row of Plot B (Fig. 2) had been the source of 639 heads of bunt out of a total of 1,266, and had produced only healthy heads in the 4th and 6th rows of Plot A and in the

3rd and 6th rows of Plot B of the same square. It will be noticed, moreover, with satisfaction that putrefied human urine, that detergent so simple and always at hand to whomsoever wishes to use it, served as an excellent remedy, since, in 1,053 heads produced by seed originally inoculated with bunt, there was not to be found a single head of bunt. One will note in examining the contents of the second rows of Plots A of the 1st and 2nd squares that the washings with ordinary water and several times repeated did not suffice to remove all of the contagious principle from the seed, for in one of the two rows there was yet to be found 15 bunted heads. One should not fail to observe how disastrous to darnel was the dust of its own smut, since of the 369 heads produced in the 2nd row of Plot C of the 2nd square 276 were stricken with the disease, while they were all free from it in the 1st and 3rd either as a natural result of the purity of the seed or as the result of the remedy applied.

Confirmation of the accuracy of what I have said will be apparent where use was made of those manures with which suspected straw was included provided the latter was so buried as to lose whatever of contagion it bore.

It will at length be recognized that the small-scale experiments agree with those carried out on a large scale, that the dust of the bunt balls is everywhere a pest when one does not counter it with something that will nullify its action, and that always it ceases to menace the seed when treated with certain preventive preparations.

It should be noted in considering the product of each row that those wherein the seed had been treated with a solution of soda or with pearl-ash lye, and particularly the 4th row of Plot A of the first square, yielded a much smaller number of heads than the neighboring rows, although in each of all the rows the same amount of seed had been sown. One must conclude that these solutions[3] were too severe on the seed; that they attacked the germ and killed part of the seed to which I applied them. There will be no doubt about that when I state that, in order to be sure that the different remedies I had used did not threaten the germinability of the wheat, I placed in orderly fashion on a piece of water-

[3] They were the product of 1 pound of ashes and of 2 pints of water reduced to 3 half-septiers after having been boiled and filtered.

soaked flannel some kernels of each of the lots of wheat I had treated and that I followed their thus exposed germination. Several kernels that had passed through the soda solution and the pearl-ash lye swelled extraordinarily; they took on a reddish color; the milky juice with which they were filled hardened a little and became like softened chalk; the pericarp was ruptured; none of the kernels thus affected germinated. We are, however, not denying the fact that these solutions were of some value. Diluted to a certain strength they caused no injury to the spring wheat, delicate as it was, and, by exercising certain precautions, excellent protective results were obtained. Although I presume little use will be made of diluted spirits of nitre as an agent for the treatment of bunt-infested seed, I must, nevertheless, digress to state that this remedy is likewise very active and that I have perceived, especially in relation to spring wheat, that there is something unusually penetrating about it.

The small-scale experiments, the results of which we shall now consider, are sufficient, I believe, to prove undeniably the facts they were designed to adduce; I believe they suffice to present more convincingly the facts of the same order derived from the large-scale experiments. I shall, therefore, abstain from reporting others, which I conducted either in clean moss or in small isolated areas, always with the intent of establishing the origin of the disease, or to prevent it by means of treatments. They would add nothing to what has already been presented here, and would take in several other experiments that, perhaps, have already become too numerous. However, one must make certain allowances here, as well as for the lengthy detailed account I have given pertaining to the matter I have treated, out of consideration for the popular benefit that has been my objective. It was felt that it was a matter of destroying universal prejudices, of determining the cause of a malady that, in an average year, takes, in France, a total amounting to one-twelfth[4] of our crop, of conducting

[4] There are some cantons near Troyes where the bunt of wheat is a general and common disease. M. de Bellenglise, Captain of Cavalry, wrote me from the neighborhood St. Quentin, where his estate is situated, that this malady causes much damage there. M. Crollat, procurator to the King in l'Election à Gannat, has called my attention to the fact that where he resides, and which constitutes a part of Limagne d'Auvergne, a delightful country and very fertile for wheat, the disease has been prevalent for at least 50 years. It was noted at the beginning of my dissertation, that in several Provinces there was complaint that the wheat was suffering from bunt.

some experiments, and of repeating them often enough so that the reader, weary with seeing this same cause of destruction reproduce itself without ceasing, under his very eyes; it was, finally, a matter of trying a large number of remedies, assuring myself of their merits, making them easy of execution, of taking into special account their economy of application, and of winning the confidence of the farmer by showing him that attention necessary to spare him from certain and inconsequential domestic outlay in order that he might obtain a general and important benefit.

This concern for the interests of the farmers occupied me very especially in the experiments I carried on this year. Although they ought to pay without difficulty the four or five half-pennies it costs to treat a bushel of bunt-infested wheat, since this bushel would sell at seven or eight half-pennies less than a like amount of the cleanest wheat and which it would bring on being treated; I dare not, however, believe that they would willingly incur this little expense and that they would make use of the solutions of soda, of potash, and pearl-ash lye. These materials are, in fact, worth four or five half-pennies per pound, each, and this amount of any one of them in the water bath is, as one may recall, what I used in moistening a bushel of seed for Part B of the plowed tract. I am not, therefore, insisting on the farmers' making special use of these compounds, which, in fact, are a bit dear and cannot be easily procured by everybody. Aside from the common aqueous solutions, the good effects of which we have noticed when preceded by the

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Everywhere there goes up a cry against this pest; everywhere the contagion is spreading; and nobody suspects the malign dust whence it [the pest] springs.

But, as the disease does not extend, by a great deal, over all the cantons, although it is generally present in every country, one can assume with some safety that there is only one-fourth of the Kingdom where the disease is consistently striking and one may estimate as a result of it a loss of ⅓ of the crop. On that basis it is conceivable that of 4 farmers, taken in the different Provinces of France, who have sown fields of equal fertility, and who have put in the same number of arpents, there is one whose wheat was infected with bunt and who has not harvested, solely because of this, more than 400 bushels of grain, while the other 3 obtained 600. That is to say, in summing up, these 4 farmers as a whole lost 1/12, or a little more than 8 per cent, of their total possible crop. There will, therefore, be available 2200 bushels of grain instead of 2400 that would have been had the wheat of the farmers not been stricken with bunt. M. Crollat observed that this disease had been prevalent for at least 50 years in the Canton whence he wrote. It has its noticeable beginnings, as is very obvious, like all pestilential diseases: It is noted down almost at the time it is observed; perhaps it was not previously recognized. A bushel of contaminated seed once introduced into Limagne established there, so to speak, the malady; and why would one not suspect that the disease, so considerable in France today, had its origin solely in a principle that was weak and absolutely foreign to the clime?

several baths in ordinary wash water, I urge the use of the putrefied urines: There is no danger of their injuring the seed; they cost nothing; and will have surely won the praise of those who have followed my advice. I shall soon be ready to tell them of the success I shall have experienced through the use of more or less concentrated greenwood-ash lye combined with lime. From the 8 combinations to which I have limited myself it will develop that the most expensive one will require an outlay of one *sol*, eight *deniers* per bushel and that the least expensive will be two *deniers*. Common ashes and lime can be had anywhere; the solutions can be made without difficulty; the treatment of the seed will be easy; and some experiments, the details of which have seen seen, seem to promise that this sort of remedy will produce the end sought.

FINIS

Original was from the Press of Guerin & Delatour.

APPROVAL

I HAVE read, on the order of Monseigneur the Chancellor, a manuscript that has for its title: *Series of Experiments and Reflections on the Cause of destruction of the Grains of Wheat in the Head*, etc. and I have found nothing therein that should prevent its being printed. At Paris, the 5th of May, 1755. GRAVES.

AUTHORITY OF THE KING

LOUIS, BY THE GRACE OF GOD, KING OF FRANCE AND OF NAVARRE: To our beloved and faithful Councilors, the Officers holding our sessions of Parliament, Masters of the common Petitions of our Palace, Grand Council, Marshal of Paris, Bailiffs, Seneschals, their Civil Lieutenants, and to others of our Justiciary to whom it will appertain:

GREETING. Our much esteemed Sr. . . . We have shown that it would be desirable to print and to give to the Public a Work entitled Series of reflections relative to the Dissertation on the cause of the destruction of the grains of Wheat, etc. by M. Tillet, Director of the Mint of Troyes; if it please you to accord to him your Letters of Authority necessary thereto: TO THESE CAUSES, wishing to treat the writer favorably, we have permitted him and do permit by these Presents, to cause to be printed the said Work as many times as may to him seem desirable, and to sell it, cause it to be sold, and distribute it throughout our Kingdom for six consecutive years from the date of these Presents. We prohibit all Publishers, Booksellers, and other persons of whatever kind or condition they may be from introducing by a foreign edition into any part of our dominion as well also as to reprint the said work and sell it, cause it to be sold, distribute it or counterfeit it under whatsoever pretext without the express permission and by written decree of the author or of those who may act in his right, on pain of confiscation of the counterfeit copies and payment of a penalty of £3000 imposed upon each offender one-third of which will revert to us, one-third to the principal hospital of the town, and the other third to the author or to others in his right, and payment of all expenses, damages, and interest. On condition that these Presents will be registered at full length upon the Register of the Community (corporate body) of Printers and Booksellers of Paris within three months from this date; that the publication of said work shall be done in our Kingdom and in no other, on good paper and with excellent characters, in accordance with the printed page, attached for a model under the counter seal of the Presents, and that the Candidate conform to all Laws of the Bookseller and especially to that of the 10th of April, 1725; and that, before offering for sale the manuscript which will have served as copy for said Work, it be returned in the same condition to the office where Authorization will have been granted, into the hands of our very

esteemed and faithful Chevalier, Chancellor of France le Sieur De Lamoignon; and that there will next be delivered two copies of said manuscript to our Public Library, one to that of our Chateau du Louvre, one to that of our very esteemed and faithful Chevalier, Chancellor of France, le Sieur De Lamoignon, and one to that of our very esteemed and faithful Chevalier, Keeper of the Seals of France, le Sieur De Machault, Commander of our Orders; the whole on pain of the nullification of these presents; of the contents of which we order and enjoin you to do to please the Candidate and his assigns having cause fully and peaceably, without allowing it to cause them any trouble or hindrance. We desire that the copy of said Presents, which will be printed at length from beginning to end of said work, be held for one duly declared and that to the copies collated by one of our esteemed and faithful Counsellors and Secretaries proof be added as to the original. We first command our Bailiff or Sergeant on this order to put into effect the execution of all those required and necessary acts without requesting other permission and in spite of Clameur de Haro [the hue and cry], Charter of Normandy, and letters to the contrary. For such is our pleasure. Committed at Versailles, the twenty-second day of the month of April, the year of Grace one thousand seven hundred and fifty-five, and of our Reign the fortieth. By the King in his Council.

PERRIN.

Recorded on Register thirteen of the Royal and Syndical Chamber of Printers and Booksellers of Paris, no. 521, folio 406, in conformity with the Laws of 1723, which prohibit, Art. IV, all persons of whatever character and condition other than printers and booksellers from selling, retailing, or advertising any books for sale in their name, whether permitted by the authors or otherwise; on condition of furnishing the models prescribed by Article CVIII of the same law.

At Paris, the 9th of May, 1755.

DIDOT, *Syndic.*